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ART. XLIX.—*Respiration of Plants;* by W. P. WILSON, S.D.

THE process of breathing in animals and plants is essentially the same. In both the free oxygen from the air, or surrounding medium, is carried into the living tissues, where, after a series of chemical changes, carbonic acid, water and perhaps some minor compounds are evolved as excretory products.

The changes in the living cell, alike in animals and plants, determine the amount of oxygen taken up and the products given off. In both an increase of temperature is a direct effect of respiration.

In this process of respiration, for plants as well as animals, a certain amount of absolutely necessary force is secured for supplying the life-functions of the organism. If either the living animal- or plant-cell be deprived of oxygen for any considerable length of time the life activities cease, and, later, death ensues. This oxygen-respiration is then a constant need of the living cell, whether it be of plant or animal.

When plants possessing chlorophyll are exposed to the sunlight they carry on a process of assimilation, in which organic substance is produced from carbonic acid and water, and oxygen set free. The amount of carbonic acid decomposed and of oxygen given off, in this preparation of organic food for the plant, far exceeds, as a rule, the carbonic acid excreted and the oxygen taken up in respiration. Thus it happens that the process of respiration, in plants containing chlorophyll and exposed to the light, is a hidden one, and difficult of demonstration.

When plants are deprived of every trace of free oxygen by placing them in an atmosphere of nitrogen or of hydrogen, they still continue, so long as life exists, to excrete carbonic acid. The discovery of this fact, which has been observed for animals as well as plants, is not of recent date, although its signification was only recently understood. As early as 1798 Rollo* observed that barley, which had been wet for two days and then placed in a vacuum, excreted carbonic acid; and that at the end of twelve days, the amount equaled six times the volume of the barley.

Saussure† placed plants in an atmosphere of nitrogen to see if they would grow, and found that they excreted carbonic acid, which he remarks must have been produced at the expense of the substance of the plants.

At a still later date Bérard,‡ while experimenting on the preservation of fruits, placed green pears in a vacuum and observed for several days following an excretion of carbonic acid.

Broughton§ and also W. Pfeffer‖ have recorded the same phenomenon in plants deprived of free oxygen.

In 1875, Pflüger¶ obtained some very interesting results in animal respiration while experimenting on frogs. These animals, placed in an atmosphere of nitrogen, were found during the first five hours and a half to expire nearly the same amount of carbonic acid as if they had been placed in pure oxygen. In another of Pflüger's experiments, a frog continued to give off carbonic acid during eleven consecutive hours, and at the end of that time had apparently lost none of its normal powers. Pflüger showed that these chemical activities, which result in the expiration of carbonic acid in the absence of free oxygen, take place in the cells. He named the process *intramolecular respiration*.

In 1878 a critical résumé on the nature and signification of respiration in plants,** written by W. Pfeffer, brought together all the then known facts which could be made to bear on this subject. The similarity existing between animal and plant respiration was sharply noted, and the term *intramolecular respiration*, taken from Pflüger, was made to cover a like series of phenomena in plant life, viz: the chemical changes continu-

* Annales de Chimie, 1798, vol. xxv, p. 42.

† Recherches Chimiques, 1804, p. 201.

‡ Annal. de Chimie et de Physique, vol. xvi, 1821, p. 174.

§ Bot. Zeit. 1870, p. 647.

‖ Arbeit. d. Würzburger Instituts, 1871, Bd. I, p. 34.

¶ Archiv. für Physiologie, Bd. X, 1875, p. 313. Pflüger gives other references to investigators in animal physiology who had made similar experiments on frogs, dating back into the 18th century.

** Das Wesen und die Bedeutung der Athmung in der Pflanze, Landwirthschaftliche Jahrbücher, 1878, Bd. VII, p. 805.

ing to take place in the plant-cell after oxygen has been withdrawn, resulting always in the production of carbonic acid, and often in other more or less important products. In this article the author lays great stress on *intramolecular respiration* as being the *first cause* of the *oxygen* or *normal* respiration.

One of the next works of any special importance on plant respiration appeared in 1879.* Wortmann measured and compared the carbonic acid expired from germinating plants placed in common air, with that given off from an equal number of plants placed in a vacuum.

He states that for short periods of time (from one to three hours), the presence or absence of free oxygen makes absolutely no difference in the amount of excreted carbonic acid gas. On the basis of these results he proposed a new theory, viz: that all the carbonic acid produced in plant respiration has its origin in intramolecular decompositions; or, in other words, that the free oxygen of the air takes no direct part in the formation of the carbonic acid in respiration.

The following paragraphs consider briefly one phase of intramolecular respiration and give in very condensed abstract a series of experiments which will later be published in full.†

The main question, here discussed, is whether the carbonic acid excreted in plant-respiration *is or is not a partial product of direct oxidation from the free oxygen of the air.*

To decide this question, quantitative measurements were made of the expired carbonic acid from the same plants placed both in air and in hydrogen gas. The methods of experimentation used were the following: a constant current of air, previously freed from carbonic acid, was passed by means of an aspirator, first through a large number of germinating seeds, or other objects to be experimented upon, and then through long tubes filled with barium hydrate in which the carbonic acid given off by the seedlings was absorbed, essentially in accordance with Pettenkofer's method.‡ After a given time had elapsed, hydrogen, as an indifferent gas, was used to replace the air. This was quickly done by exhausting the air in the seed-vessel and tubes leading thereto, and then introducing hydrogen to take its place. A constant current of hydrogen was then passed over the seedlings, precisely as had been done previously with air.

The carbonic acid was determined for either half hourly or hourly intervals. This was easily accomplished by using two

* Wortmann, über die Beziehungen der intramolecularen zur normalen Athmung der Pflanze, in Arbeit d. Würzburg Instituts, 1880, Bd. II, p. 500.

† A much shorter abstract than the present appeared in German in "Flora," No. 6, 1882.

‡ Abhandlungen der Akademie d. Wissenschaften in München, Cl. II, Bd. IX, Abth. II, p. 229.

absorption tubes filled with barium hydrate, and so connected together by glass stopcocks that the current of air or gas, when passing through one tube, could be instantly turned through the other without interruption. Thus while the current passed through one tube, the other could be removed, emptied, refilled and replaced for the next half hour's determination.

In conducting an experiment, measurements were made first in air for several half hours, then the air was replaced by hydrogen, and after an equal number of half hours, for hydrogen was again substituted air, in which the third series of determinations took place. The last series in air was used to compare with the first, and it readily showed whether the seedlings had undergone any lasting change for want of oxygen. The temperature of the plants for any given experiment was not allowed to vary; also the current of air or gas was held constant for all the successive half hours.

A great advantage in the methods here adopted lay in subjecting the same seedlings, first to air, and then to hydrogen; thus the products of the respiration of the same plants under these two different conditions could be compared.*

A large number of seedlings were employed (from one to several hundred, according to size), thus ensuring the production of a comparatively large amount of carbonic acid in a short space of time, with which much more accurate measurements could be made than with small quantities. The amount of carbonic acid was determined by the volumetric method.

According to the investigations of Wortmann with seedlings of *Vicia Faba*, *Phaseolus multiflorus* and some others, the carbonic acid excreted in short spaces of time, one to three hours, was found to be the same whether oxygen was present or not.†

In the case of seedlings of *Vicia Faba*, with which Wortmann mostly experimented, I have found this to be substantially true; for all other germinating plants, flowers, or parts of plants there was an immediate decrease in the carbonic acid produced when oxygen was excluded. The first half hour's respiration in hydrogen often yielded but one third or even one-fourth the carbonic acid produced by the preceding half hour's respiration in air. After three or four consecutive half hours of intramolecular respiration, sometimes from the very first, a steady decrease in the carbonic acid was recorded for every following half hour. This was also found to be true for *Vicia Faba*. The following are the results obtained from

* Wortmann used two sets of plants, as nearly alike as selection would permit, one in vacuum compared with one in air.

† "Als ich jedoch die in ganz kurzen Zeiträumen—nach der ersten, zweiten und dritten Stunde—ausgeschiedenen Kohlensäuremengen mit einander verglich, so fand ich jedesmal, 'dass das in dieser Zeit durch intramoleculare Athmung ausgeschiedene Kohlensäurevolumen dem durch normale Athmung erzeugten gleich ist.'" Wortmann, l. c., p. 509.

Lupinus luteus.

I. Period.	Air.	1st half hour,	5·7 mgms.	CO_2.
		2d half hour,	6·6 "	"
II. Period.	Hydrogen.	3d half hour,	1·5 "	"
		4th half hour,	1·5 "	"
III. Period.	Air.	5th half hour,	3·9 "	"
		6th half hour,	5·7 "	"

The comparative decrease in the amount of expired carbonic acid in hydrogen is here greater than with many plants. The following fungus shows much less difference:

Cantharellus cibarius.

I. Period.	Air.	1 hour	=16·20 mgms.	CO_2.
II. Period.	Hydrogen.	½ hour=5·28	=10·80 "	"
		½ hour=5·52		
III. Period.	Air.	1 hour	=16·20 "	"

It will be seen that with *Lupinus luteus* less carbonic acid was measured in the fifth than in the sixth half hour. This is partially based upon the fact that at the beginning of the fifth half hour the seed-vessels contained less carbonic acid than at the beginning of the sixth. With a constant current of air and a constant production of carbonic acid, it requires a little time after the air or gas has been changed before an equilibrium between the produced and the out-flowing gas establishes itself.

Among a large variety of germinating plants, branches with leaves, flowers, fruits and fungi, *Vicia Faba* has been the only plant which has given results differing from the above.

That the volume of carbonic acid expired in normal and in intramolecular respiration is the same, is, therefore, in accordance with the facts above stated, untrue. The theory which Wortmann founded upon this fallacy, viz: that the total volume of carbonic acid excreted in plant-respiration has its origin absolutely independent of the oxygen of the air, in intramolecular decompositions, falls also to the ground.

Moreover, if it had been found by experiment, that the volume of carbonic acid given off in intramolecular equaled that of normal respiration, Wortmann's theory would still fail.

This has been well shown by W. Pfeffer, in saying, substantially: if an equal amount of carbonic acid were formed in both intramolecular and normal respiration this would only prove that the same number of carbon affinities for oxygen had been satisfied in each case, would in no way indicate from whence the supply of oxygen came. And in case free oxygen was active in normal respiration, still in intramolecular, when free oxygen was absent, the full supply might yet be obtained through constant powerful attractive forces which could take

oxygen from other combinations, and in this way give rise to secondary changes.*

Partial presence of Oxygen.—When seedlings of *Helianthus annuus* are placed in a current of common air for a given period of time, and then for the same length of time are exposed to a current of air made up of a mixture of one-fifth air and fourth-fifths hydrogen, a comparison of the excreted carbonic acid in both cases shows no difference whatever. Sufficient oxygen is present in the mixture to supply all demands made by the plants. When, however, the expired carbonic acid in the respiration of plants, placed first in air, and then in a mixture of $\frac{1}{20}$ air and $\frac{19}{20}$ hydrogen, is compared, one finds the amount much greater during respiration in air. Thus a trifle over one per cent of oxygen in the air is not sufficient to supply the demands for this gas made by such seedlings.

Influence of light on respiration.—The presence of light does not in any appreciable degree directly affect the amount of carbonic acid given off in the respiration of plants. I have experimented on Orobanche, Monotropa, Fungi and seedlings of several kinds. Neither in air nor in hydrogen gas, for plants placed in the light or in the dark, is there any difference in the excreted carbonic acid which can be measured.†

Tübingen, Würtemberg. April, 1882.

ART. L.—*On the Question of Electrification by Evaporation;* by S. H. FREEMAN, Fellow in Physics in Johns Hopkins University, Baltimore, Md.

IT has been very commonly believed that evaporation is an important source of atmospheric electricity. So far as this belief has an experimental basis, it is to be found in the researches of Pouillet,‡ and of Tait and Wanklyn.§ In comparison with their work the experiments of Volta, Saussure and others are of little value. As the result of an elaborate series of experiments Pouillet came to the following conclusions:

1. Simple changes of state never give the least sign of electrification.

* Denn daraus folgt nur, dass in beiden Fällen gleichviel Affinitäten des Kohlenstoffs mit Sauerstoff gesättigt werden, nicht woher dieser stammt, und falls der freie Sauerstoff mitwirkte, könnten beim Fehlen dieses die fortdauernden, mächtigen, Anziehungskräfte nunmehr ihre volle Befriedigung finden, indem sie durch Entreissung von Sauerstoff aus anderweitigen Verbindungen eben die Veranlassung zu sekundären Prozessen werden. Pfeffer, Pflanzenphysiologie, Bd. I, 1881, p. 371.

† The methods of analysis which I have used allowed the detection of one-tenth of a milligram of carbonic acid without difficulty.

‡ Annales de Chimie et de Physique, II, tome xxxv, p. 401, and xxxvi, p. 4.

§ Phil. Mag., IV, vol. xxiii, p. 494.

2. From solutions of alkaline solids the water evaporated carries away a negative charge.

3. From solutions of gases, acids or salts the water carries off a positive charge.

Tait and Wanklyn repeated Pouillet's experiments with a much more sensitive electrometer. They detected electrification where Pouillet had failed to do so, e. g., in the evaporation of distilled water; and in some other cases the effects obtained were totally different in kind and degree from those obtained by Pouillet and earlier experimenters.

In these experiments a few drops of liquid were dashed on a red hot platinum dish insulated and connected with an electrometer. So long as the spheroidal state continued, little or no electrification was observed; but as soon as violent vaporization began, decided deflections were obtained. This fact and other circumstances about the experiments indicate that friction was the chief source of electrification and make it doubtful whether evaporation had any part in producing the observed electrification. Tait and Wanklyn recognized the fact that to friction was due a large part of the electrification which they obtained. Still, in his lecture on Thunder Storms,* Professor Tait, speaking of atmospheric electricity, says: "In calm, clear weather the atmospheric charge is usually positive. This is very commonly attributed to evaporation of water, and I see no reason to doubt that the phenomena are closely connected." After reading these lectures the writer of this article decided to investigate whether evaporation alone and under normal conditions causes electrification, and whether such electrification, if any, is sufficient to account for the phenomena of atmospheric electricity. In this connection it will be remembered that Faraday traced the electricity, produced by the escape of steam or water from orifices, to friction, and concluded it was not due to evaporation, nor did it have any bearing on atmospheric electricity.

The investigation was begun with the expectation on the part of the writer of finding electrification. Professor Rowland, to whom the author is much indebted, believed that no electrification would be found. In any case the subject seemed important enough to warrant experiments which were free from the objections which hold against those already described. At the very beginning of the investigation it was found that at most the electrification due to evaporation would be extremely small and very great caution would be necessary to eliminate various sources of error. The apparatus finally used consisted of an evaporating dish 19^{cm} in diameter and 1^{cm} deep, insulated, and connected with one pair of quadrants of a Thomson's quadrant electrometer. The other pair of quadrants and the case of

* Nature, vol. xxii, p. 340.

the electrometer were connected with earth. By means of a mercury commutator the evaporating dish and its connections could also be connected with earth. The evaporating dish was supported on a wire frame suspended by silk threads. To prevent disturbances due to electricity on neighboring objects, particularly the clothes of the observer, it was found necessary to enclose the whole apparatus in a metallic case. Openings were made in this case to facilitate evaporation and to permit the mirror of the electrometer to be seen. The commutator was governed by a silk thread passing through an opening in the case. In the experiments, the results of which are given in table II, the evaporating dish was of copper; in the other cases it was of tin. The liquids examined were alcohol, sulphuric ether, water, and solutions of NaCl and of $CuSO_4$. The last three were most carefully studied, water and NaCl because of their importance in evaporation in nature, and $CuSO_4$ because of the very large deflections which Tait and Wanklyn obtained with it.

The electrometer was about thirteen times as sensitive as Professor Tait's. Its deflections were reduced to absolute measure by comparison with a Daniell's cell, the deflections produced by which were observed before and after each series. The capacity of the evaporating dish and its connections was found by comparison with Professor Rowland's standard condenser to be 65^{cm} nearly. The rate of evaporation was determined by weighing the evaporating dish and liquid before and after each series. It was found impossible to eliminate every source of electricity except evaporation, since deflections were always obtained even with the evaporating dish dry. In order to correct for this each series began and ended with two or more observations of the deflections obtained by insulating for a known time, usually five minutes, the dry evaporating dish. Between these observations several similar observations with the liquid evaporating were made.

As the result of a large number of observations in which the evaporating dish was insulated for five minutes:

Alcohol gave a potential from	−·04 to −·09	that of a Daniell's cell.	
Cotton wet with alcohol gave a potential from	+·02 to −·06	"	"
Sulphuric ether gave a potential from	−·02 to −·10	"	"

(The sign indicates the charge apparently carried away by the vapor.) No correction has been made in the above figures for the deflections obtained with the dry dish. These were more variable, but usually of the same sign, as when a liquid was evaporating, and of a magnitude not very different. Making this necessary correction—

Alcohol gave a potential from	−·024 to −·065	that of a Daniell's cell.	
Cotton wet with alcohol gave pot'l from	−·028 to −·050	"	"
Sulphuric ether gave a potential from	−·004 to −·023	"	"

In the above observations the rate of evaporation was not measured.

The earlier observations on water and salt water gave deflections about 0·1 that given by a Daniell's cell, the deflections with the dry dish being nearly the same (see table I). Though the dish was insulated for five minutes or more, the position of the spot of light was read every 30 seconds. Far the greater part of the deflection was attained in the first minute or two, and after five minutes scarcely any further deflection could be discovered.

The observations thus indicated that the small deflections obtained were mainly due to leakage from electrified parts of the apparatus. The sources of this leakage were carefully investigated and three principal ones were discovered, viz: the charged needle of the electrometer, the vulcanite posts supporting the electrodes of the electrometer, and the glass cup of the commutator. In the later experiments care was taken to eliminate these sources of leakage as far as possible. The connections were so made, that the electrodes and commutator were not touched for days before the experiments, and the charge on the needle was not replenished within about 24 hours of the experiment. When these precautions were taken, the deflections became less than 0·01, and generally less than 0·005 of a Daniell's cell (see table II).

In contrast with these deflections, Tait and Wanklyn obtained by dashing liquids on red hot platinum, for

Solution $CuSO_4$ a potential	+300	times	that of a	Daniell's cell.
Solution NaCl a potential	+120	"	"	"
Distilled water a potential	+ 24	"	"	"
Alcohol	+ 3	"	"	"

In order to compare the deflections obtained by evaporation with those obtained by friction, the experiments of Tait and Wanklyn were repeated. It was impossible, however, to measure the deflections obtained, since in almost every case, as soon as the spheroidal state ceased, the spot of light went beyond the limits of the scale.

To show the sensitiveness of the apparatus to very slight friction it was so arranged that the drops of liquid could fall upon the evaporating dish from an insulated conductor in metallic connection with it. A few drops of liquid, falling 3 or 4^{cm}, were sufficient to give deflections of nearly the size obtained from a Daniell's cell and much larger than any obtained in evaporation.

In order to compare the results obtained with the phenomena of atmospheric electricity the writer computed the virtual depth of water which would be necessary to produce one flash of lightning, if the vapor in a thunder-cloud had been electrified as indicated by the experiments. By virtual

depth is meant the depth of the layer of water over the whole area of the cloud, were all its vapor condensed to the liquid state.

Let Δ = this depth in c.m. for a cloud of area A and height a.

Let V and C = respectively the potential and capacity of the cloud.

Let q = the charge per gram of water.

Then

$$\Delta = \frac{VC}{Aq}$$

But from Sir Wm. Thomson's experiments $V = 130a$,* and by the ordinary formula for a condenser of horizontal plates

$$C = \frac{A}{4\pi a}$$

Now in the experiments,

Let d = the deflection obtained by insulating for t minutes, and apparently due to evaporation.

Let D = the deflection obtained from a Daniell's cell.

Let W = weight in grams of liquid evaporated in T minutes.

Let v and c = potential and capacity of evaporating dish and its connections.

$$\therefore q = \frac{vcT}{tW}$$

But $c = 65$ c.m. as stated above; and $v = 0{\cdot}00374\frac{d}{D}$ in electrostatic units according to Sir Wm. Thomson's measurements.†

$\therefore \Delta = 43\frac{tWD}{Td}$ in terms of the original measurements in the experiments.

I. *Table showing results of earlier series.*

Liquid.	Deflection with evaporation.	Deflection with dry dish.	d in scale divisions.	D in scale divisions.	t in minutes.	T in minutes.	W in grams.	Δ in c.m.	Sign of apparent charge of vapor.
Distilled water	4·12	4·60	+0·48	42·02	5	43	1·44	630	+
Distilled water	4·82	4·33	−0·49	42·05	5	43	0·49	210	−
Distilled water	3·95	4·50	+0·55	41·55	5	37	0·54	237	+
Distilled water	2·48	2·33	−0·15	42·00	5	41	0·80	1170	−
Solution NaCl	6·70‡	4·68	−2·02	41·75	5	55	0·29	24	−
Solution NaCl	3.47	4·40	+0·93	41·50	5	54	0·48	85	+
Solution NaCl	3·10	3·94	+0·84	42·20	5	39	0·70	194	+

* Papers on Electricity and Magnetism, p. 259.

† Papers on Electricity and Magnetism, p. 245.

‡ Throughout this series there was an instantaneous deflection at breaking contact, which was several times larger than the total deflection obtained during the 5 min. insulation. The series therefore is of much less value than any of the others in which the instantaneous deflection was never more than 0·1 of a division.

II. *Table showing results of later series.*

Liquid.	Deflection with evaporation.	Deflection with dry dish.	d in scale divisions.	D in scale divisions.	t in minutes.	T in minutes.	W in grams.	Δ in c.m.	Sign of apparent charge of vapor.
Saturated solution $CuSO_4$	0·3	0·0	+0·3	37·6	2	174	3·19	198	+
"	0·12	0·6	+0·6	37·6	2	174	3·19	99	+
"	0·8	0·6	+0·2	37·6	10	174	3·19	1482	+
"	0·6	0·42	+0·18	39·6	10	38	0·9	2240	+
"	0·5	0·21	+0·29	39·6	5	38	0·9	695	+
"	0·1	0·21	−0·11	39·6	5	38	0·9	1833	−
"	0·3	0·21	+0·09	39.6	5	38	0·9	2240	+
Saturated solution NaCl	0·0	0·35	−0·35	39·6	6	30	0·54	525	−
"	0·2	0·12	+0·08	39·6	2	30	0·54	766	+
"	0·4	0·35	+0·05	39·6	6	30	0·54	3678	+
Hydrant water	1·6	0·6	+1·00	37·6	10	30	0·59	318	+
"	1·45	0·6	+0·85	37·6	10	30	0·59	373	+
"	1·3	0·6	+0·70	37·6	10	30	0·59	454	+
Distilled water	0·5	0·6	−0·10	39·6	7	35	0·89	3031	−
Cotton wet with hydrant water	1·0	0·66	+0·34	37·6	11	30*	0·59*	1029	+

The actual value of Δ (the virtual depth of water) for a cloud 1500 meters thick, if condensation began at 35° C. is about 5·9 c.m.; at 30° C. is about 4·5 c.m.; at 25° C. is about 3·4 c.m.; at 20° C. is about 2·6 c.m.; at 15° C. is about 1·9 c.m.

Hence, even if the deflections d of the tables are really due to evaporation, and if evaporation be the principal source of the atmospheric electricity, the quantity of water, which would be required to produce a single flash of lightning, is very much greater than the actual quantity ever found in a thunder-cloud, while a thunder-cloud usually gives not one but many flashes.

It may be objected to this reasoning that a thunder-cloud in some way collects the electricity obtained by the evaporation of a much larger quantity of water than that contained in itself. On this account it is of interest to consider the total evaporation on the globe. If 100 c.m. be taken as the average annual rainfall for the whole surface of the earth and 400 c.m. be taken as the value of Δ, the total annual evaporation on the earth is sufficient to produce only about 14,000,000 flashes of lightning from clouds nine square kilometers in extent. Hence if one were able to observe every flash of lightning which occurs within eighteen kilometers of himself the average number of lightning flashes seen by such observers at different points of the earth's surface in one year would be only twenty-eight. Calculation from more exact data would give a less

* In this case the rate of evaporation was not measured, but was assumed in computing Δ to be the same as for hydrant water at the same time. The rate was probably greater, which would make Δ still greater.

number. This calculation assumes that all the electricity is discharged through lightning flashes, whereas a very large portion of the atmospheric electricity is discharged in other ways, e. g. in the aurora borealis and other atmospheric phenomena which are referred to electricity and in quiet discharges which take place without any noticeable accompanying phenomena.

Evaporation is then at most a very insignificant source of the atmospheric electricity.

But further, the following facts are to be observed:

1. The deflections obtained in the experiments were always very small. (In the original readings it was easy to make an error of 0·1 a division, and though the numbers in the tables are the means of several observations, they are still affected by this error of reading.)

2. The sign of the apparent charge is not always the same for the same liquid. This is particularly noticeable in the important case of water.

3. The deflections were very much diminished when care was taken to eliminate very small sources of electricity. Still the deflections, obtained with the evaporating dish dry, are usually much larger than the difference between these and the deflections, obtained when a liquid was evaporating.

Evidently then most of the electrification was due to other causes than evaporation, and the experiments do not certainly trace any electricity whatever to this source.

The problem of the source of the electricity of the atmosphere is still unsolved. Evaporation, first proposed by Volta, whose theory until now has been better supported by experiment than any other, fails to account even for a small portion of it; and no other source has been proposed, which can as yet be considered sufficient.

ART. LI.—*Observations on Snow and Ice under pressure at Temperatures below 32° F.;* by EDWARD HUNGERFORD.

THE following experiments on the behavior of snow and ice under pressure, were conducted during the successive winters from 1869 to 1871. They have never been given in detail to the public. At the meeting of the American Association for the Advancement of Science, held in Burlington, August, 1867, I gave a résumé of the observations made in the previous winter or spring, under the title of "A Preliminary Notice of Experiments on Snow at Temperatures below 32° F." Of this paper a short abstract appears on page 39 of the proceedings of the Association for that year. I also gave at the Chicago meeting,

of the following year, some account of the work done in the winter preceding. On neither of these occasions were details of observations given to the press. The work was incomplete, and as even at my home in Vermont, the periods of extreme cold, lasting for two or three days, in which the most favorable opportunities are given for conducting such experiments, do not occur many times in any one winter, my work ran along until the spring of 1871. My note-books show an accumulation of numerous trials of the effect of pressure upon snow and ice, only a portion of which will be included in this article.

After the observations were made, a change of residence for a number of years, took me away from Vermont to a region less favorable for such observations. Other occupations have also prevented me from putting the results into presentable shape. In the meantime, however, these results have not, so far as I know, been anticipated, and though they are not by any means as satisfactory as I could wish, the publication of them may at least save anyone else the necessity of going over the same ground. I give first in order a series of experiments with prisms of ice, transversely fractured, and welded again under a relatively light pressure, at various temperatures ranging from 26° F., down to 9° F., and a series of experiments on snow under pressure in an iron press.

A few words as to the object sought in this investigation. In the first place I wished to ascertain whether fractured masses of ice, when brought together at temperatures considerably lower than the ordinary freezing point of water, will be firmly reunited under a moderate or even a heavy pressure. Also, whether cold and dry snow will under like circumstances of pressure become converted into solid ice.

Thirdly, I have considered it desirable to note whether the time element intervenes as a factor, whether reunion and glaciation at low temperatures take place instantaneously, or whether the process requires time for its completion.

And in the fourth place, it is a matter of interest and perhaps of practical bearing on theories of glacier motion, to know whether, in case such union of ice masses and snow granules actually does take place at low temperatures, we are to attribute the mending of the ice joint and the glaciation of the snow to regelation, on the theory advanced originally by Faraday,* or whether the union is a simple result of the plasticity of the ice retained at low temperatures. The supposition of regelation necessarily involves the development of moisture along the ice joint and among the snow granules. In the absence of such moisture the result is to be ascribed simply to the molec-

* The observation was Faraday's, the term regelation was applied to the observed facts by Professor Tyndall.

ular plasticity of ice, whether in the mass or in the form of minute snow crystals. The mention of these points, around which the interest of the investigation gathers, will enable the reader to follow the details more intelligently.

My method of conducting the trials with prisms of ice is as follows: The prisms are obtained by sawing a block of lake ice into slabs an inch or two thick, and again sawing the slabs into prisms three or four inches long. The prisms are then transversely broken. A couple of small, wooden clamps, such as carpenters use, each with two light wooden screws are provided, and the two inner faces of each clamp are lined with emery cloth, to keep the ends of the prisms from slipping when the pressure is brought to bear upon them. In such a clamp, which should be light (made with, perhaps, half inch wooden screws, and the opposing pieces not more than a foot long, by a half inch thick and three inches wide), two prisms can be treated at the same time. The fractured faces of the prism are brought together, and when they have been carefully adjusted, the clamp may be laid over the ends of the two prisms which lie parallel and near to each other on the table. The screws are then gently tightened. To avoid heat and moisture the handling should be done, if they are handled at all, with a cold cloth; or they may be pushed about with a chilled knife blade to get them into position. All my work was done in a cold shed, which was kept open to the weather. It will be noticed that in these trials, both with prims and with snow, the temperatures were carefully noted, at frequent intervals, during the progress of the trial. These are the natural temperatures of the room where the work was done, and so of all the apparatus concerned in the experiments.

With these explanations I submit several illustrations of the behavior of ice prisms under a light pressure. These are in the form of abstracts made directly from my note-books, with only such corrections and explanations as seem necessary to a full understanding of each trial.

JANUARY 27th, 1869.—Temperature ranged from 12° F. to 22° F. during the following trials, which began on the 26th. Prisms of ice an inch thick by two inches were broken, and after matching them on the lines of fracture, were placed in clamps and subjected to a moderate pressure, the screws being tightened several times during the experiment. One prism was parted on the evening of the 26th and showed evident signs of reuniting.

The one that was permitted to remain in the clamps over night, therefore, some twenty-four hours, was so firmly reunited that in endeavoring to part it I thought the ice would break elsewhere. It finally yielded on the old joint to a force little less than sufficient to break the ice in any direction. There had evidently been a firm reunion, not a mere sticking together but a knitting of the joint.

FEBRUARY 3d, 1869.—Temperatures for Feb. 2d to Feb. 3d. Feb. 2d, 10 A. M. 7° F., 5 30 P. M. 13° F., 9 P. M. 11° F., 9 A. M. of Feb. 3d, 20° F. Highest temp., 20° F. The day of the 2d of February was bright but cold.

The prism of ice remained in the clamps with only moderate pressure for nearly twenty-four hours. The joint was firmly knit at the end of that time, separating with a snap under a strain, showing clear signs of actual welding. Proof that the union is not due to atmospheric pressure is furnished by the great ease with which the joint is separated when the pressure has been applied only a few minutes.

FEBRUARY 8th, 1869.—Temperatures, 9 A. M. 2° F., 1 P. M. 19° F., 2.15 P. M. 20° F., 4 P. M. 22° F., 6.30 P. M. 20° F., 9.15 A. M., of Feb. 9th, 21° F., 10.30 A. M. 24° F. Highest temp., 24° F. A fine block of ice two inches square was secured in clamps at 9 A. M. of the 8th. Closed experiment at 10.30 A. M. of the 9th. The block separated on the joint with difficulty under a smart blow.

Feb. 21st, 1870.—Temperatures, 1.30 P. M. 21° F., 7.30 P. M. 12° F., 7 A. M., of Feb. 22d, 6° F., 2 P. M. 13 F., 3 P. M. 16° F. Highest temperature 21° F. Set three blocks in clamps and one without clamps.

No. 1 which had stood without clamps was parted with a light snap and readily.

No. 2, twenty-four hours in clamps, was crushed by the pincers in applying transverse strain, and yielded near to, but not on, the old line of fracture. It was shivered into fragments but the two faces were left firmly adhering.

Nos. 3 and 4 parted readily after lying in clamps twenty-four hours. This my notes conjecture to have been due to imperfect adjustment of their faces in placing them in the clamps.

JANUARY 4th, 1871.—Temperatures, 8.15 A. M. 2° F., 12 M. 9° F., 1 P. M. 9° F., 5 P. M. 11° F., 10 P. M. 8° F., 6 A. M., Jan. 5th, 15° F., 12 30 P. M. 26° F. Highest temp., 26° F. At 4 P. M. of the 4th placed in clamps a prism of ice near an inch and a half square and four inches long. Got a very perfect joint. Continued it under pressure till noon of the 5th. The union was quite perfect. Seized the ends of prism with cloth and could not break the joint over the edge of the bench by force. Then clamped it in two clamps and wrenched it apart. It parted only with violence, the separation taking place in a new line parallel to the old line of fracture.

The following trial is selected on account of the low range of temperatures.

JANUARY 7th, 1871.—Temperatures. 11 A. M. 12° F., 12 A. M. 12° F., 2 P. M. 12° F., 6 P. M. 9° F, 10 P. M. 3° F. Highest temp., 12° F. Three ice prisms were tested in the clamps from 11 A. M. and 12 noon until 10 P. M.

Two of them parted with moderate force. The third required much more force, about the same as would be required to break a prism of the same size. After separation, the surfaces of the old joint presented the torn, ragged appearance indicative of violent tearing apart.

The note here observes that the success of the experiment depends on getting the joint of the broken prism properly readjusted, so as to make the applied pressure bear evenly on all points of the joint. Failure to do this accounts for the imperfect adherence of some specimens.

JANUARY 18th, 1871.—Temperatures. This day before noon temperature ranged from 16° F. to 18° F., 1 P. M. 16° F., 4 P. M. 15° F., 10 P. M. 6° F. Jan. 19th, 7.30 A. M. 0° F., 12 noon 14° F., 4 P. M. 22° F. Highest temp., 22° F. At about 11 A. M. of 18th put two prisms in clamps and continued one of them until 4 P. M. of the 19th. One of these much less than a square inch in cross section, was well united so as to lift sixteen pounds weight. The prism was somewhat crushed by pressure and gave way elsewhere than on the original line of fracture under a heavier weight. I finally wrenched the old joint apart, it giving way partly in a new direction.

JANUARY 10th, 1871.—Temperatures, Jan. 9th, 10.15 A. M. −4° F., 12.15 P. M. −2° F., 1.15 P. M. −2° F., 2.15 P. M. 0° F., 4 P. M. 0° F., 10 P. M. −4° F. Jan. 10th, 6 A. M. −7° F., 12 noon 8° F., 2.30 P. M. 9° F., 4.30 P. M. 8 F., 9.30 P. M. 3° F. Highest temp., 9° F. On the 9th between 11 and 12 in the morning put a prism of ice in clamps and kept it there till 4.30 of the 10th. It was so firmly reunited that it did not part when subjected to a strain of thirty pounds. An addition of five lbs. more parted it. The test was made by securing one end of the prism and lifting a weight attached to the other end. The area of the cross section of the prism was less than a square inch by actual measurement.

A glance at this series of experiments with prisms shows that fragments of ice may be firmly re-united at all temperatures down to 9° F. which is the lowest maximum to which the ice was exposed in any trial. As to the main fact of the welding of ice at such temperatures, the evidence is conclusive. In order to avoid all suspicion that atmospheric pressure may have held nicely joined fragments in place, with sufficient force to account for the result, attention is called to the fact mentioned in the note of Feb. 3, "the great ease with which the joint is separated when the pressure has been applied only a few minutes." But, not content with this, the note of January 10, 1871, institutes a test by weight and measure. In this instance the highest range of temperature is 9° F. The prism is kept under the usual pressure during twenty-nine hours, and a firm union secured. The area of cross section measures less than a square inch, and the force necessary to pull the fragments apart exceeds thirty pounds. In my notebook comments on this trial, I find the conviction expressed that by long continued pressure the joint might be completely obliterated. And I remember that this remark was occasioned by the close knitting of the joint, and the growing indistinctness of the line of fracture in certain cases, under pressure.

I scarcely need call especial attention to the violence required in some cases to part the reunited fragments or to the fact that the second break sometimes occurs along a new line of fracture as in the note of January 4th. In regard to the influence of time as a factor in the reunion of the broken prisms, my impressions are strongly in favor of the value of time to the result. I have never known a prism which had only been exposed to pressure for a few minutes to show strong adherence. It is true that many prisms after being in the clamps a long while, fail to unite, owing to improper contact or uneven pressure, as is remarked in the above notes; but imperfect contact and uneven pressure could hardly be the cause of this so uniform parting of prisms which had been in contact only a short time —freshly joined faces come apart readily with a light snap. The trial recorded under date of January 27th, 1869, gives us a prism, subjected to pressure for a few hours, which shows "evident signs of reuniting," while one which has been kept in the clamps "some twenty-four hours was so firmly reunited that in endeavoring to part it I thought the ice would break elsewhere." So far as the evidence goes therefore, the element of time is an important one in the process; and this conviction has grown upon me during the years in which I have experimented upon both snow and ice.

As to the fourth point noted as being among the objects kept in view during these studies, it is difficult to conceive

how any moisture can intervene to assist in restoring the continuity of the faces of broken prisms. In discussing the effects of heavy pressure upon snow, this subject will come up again. But in the prisms, exposed to temperatures such as some of these were exposed to, and having all their sides free to the air, while pressure was applied at the opposite ends, it is difficult to conceive of any development of moisture to effect the result. It is very true that Mr. Tyndall, whose beautiful experiments upon ice have attracted the admiration of all readers, found lines of moisture developed transversely to the direction of pressure in masses of ice. But these observations were made upon ice at 32° or near that point. Here we have temperatures of all grades from 26° F. down to 9° F. The exhibitions of those inner lines or surfaces of moisture at a temperature close upon 32°, it is scarcely necessary to say, was due to the liquefaction of ice under pressure, according to the law given by the brothers Thomson. Inasmuch as water expands in freezing, it was observed that by preventing the expansion of water through pressure, that fluid could be reduced several degrees in temperature and still be kept in the fluid state. The brothers Thomson have given us the numerical expression of this fact in the rule evolved from experiment, that for every pressure of one atmosphere bearing on the fluid, its temperature may be reduced ·0075 of a degree of Centigrade without congelation. And conversely for every atmosphere of pressure brought to bear upon a mass of ice the temperature at which moisture can be developed in the ice will be lowered by ·0075 of a degree Centigrade. This is owing to the diminished volume of the ice under the pressure. But the application of this law to the case before us is out of the question. It would require by this rule a pressure of over 1950 pounds to the square inch to liquefy ice having a temperature of 30·2° F., and for the highest maximum temperature to which the prisms were exposed (26° F.) it would require a pressure of over 6456 pounds to the square inch. With the light clamps used no more than a few atmospheres of pressure were produced. I am thus explicit in the application of the principle brought out in the rule of the brothers Thomson, because the same considerations return in future experiments with snow under pressure.

The observations on the behavior of snow at comparatively low temperatures have been much more difficult than those upon ice prisms. Heavier pressures have come into play, and it is only by special contrivances that the conversion of snow into ice can be watched. My earlier trials, those to which allusion was made at Burlington and Chicago, were conducted with wooden moulds, into which the snow was packed, and the

pressure was brought to bear by means of a square wooden plunger, driven by an iron screw, which worked in a wooden frame. For these contrivances, which were, in many respects, very useful for earlier observations, I afterwards substituted a much more convenient iron press. The details of this press, it is hardly necessary to describe. The snow was packed in a cast iron cylindrical mould, having a smooth circular bore in the center one inch in diameter. The bore is three and one-half inches deep and of smooth finish, to lessen the friction of the snow. Into this bore works a plunger, also of iron, nicely fitted. The upper end of the plunger is attached to an iron head, grooved on its sides, so as to work along the vertical iron rods which are the uprights of the press. Into the upper side of this head the lower end of the iron screw projects and works in brass fittings—while the screw itself runs through an upper iron cross bar. By this means the plunger with its head can be carried down into the mould smoothly, by a slow motion of the screw. To the head of the screw a wheel was attached to which the power was applied by means of a cord passing over a pulley. To the cord weights were hung and their amounts noted. By this means a reasonably continuous and regulated pressure could be brought to bear upon the snow in the moulds. These moulds were split vertically through the middle, and were held together by a stout iron ring, which could be struck off with a blow of a hammer. When opened, the moulds exposed a vertical section of the little cylinder of ice resulting from the pressure. The slight differential motion of the pressed mass, on each side of the plane along which the mould was split, left a corresponding plane of easy cleavage in the resultant mass. Aside from the convenience of this whole arrangement, it gave me the advantage, that the conducting power of the iron, as exposed to low temperature, would help to prevent the accumulation of heat from the gradually increased pressure and slow downward motion of the plunger. The snow in the interior of the mould would thus be kept very close to the temperature of the external air.

I am under great obligations to my friend, Mr. Robert W. Willson, Assistant Astronomer in the Observatory in New Haven, who has very kindly assisted me to obtain the working power of this little snow press by actual trials. We have tested the force exerted at the lower face of the plunger for every five pounds of power up to fifty applied at the wheel. The observations, made and repeated by us interchangeably, to correct the personal error, have resulted in a very even rate, so that we have the pressure for every five pounds of power, and are enabled to extend the rate up to a power of sixty pounds at the wheel, which is the highest used. I can therefore insert in all

of the following trials with the iron press, excepting the first in which the screw was turned by hand, the actual pressure per square inch used. The natural temperatures of the room or shed in which the work was done are also carefully recorded, along with each trial, for every few hours of the day or days through which the trial lasted. With these explanations I give the following:

Experiments on Snow with the Iron Press.

DECEMBER 29th, 1870.—Temperatures, 9 A. M. 0° F., 1 P M. 3° F.. 4 P. M. 3° F., 7 P. M. 2° F., highest temperature 3° F. At 9 o'clock A. M. packed the mould with snow and applied pressure very gradually until noon, making a quarter turn only of the screw at a time, so as to avoid evolving heat in the snow by sudden pressure. At noon the force necessary to turn the screw through the quarter had become considerable, though by no means violent. The screw was turned slowly through the quarter. Thus three hours were consumed in bringing the strong pressure to bear.

Discontinued the experiment at 7 P. M. The snow was converted throughout into perfect ice, white, clouded here with darker and there with lighter lines—translucent. This experiment is a perfect success. Everything has been manipulated with the utmost care.

JANUARY 4th, 1871.—Temperatures, 8.15 A. M. 2° F., 12 noon 9° F., 1 P. M. 9° F., 5 P. M. 11° F., 10 P. M. 8° F., 6 A. M., January 5th, 15° F., 12.30 P. M. 26° F.

Commenced experiment at 9 A. M. and discontinued at 6 A. M. of January 5th, The highest temperature therefore to which the experiment was exposed would be 15° F. Result most perfect ice. At 9 A. M. subjected snow to pressure in new iron machine. Applied only very slight pressure gradually for some time by hand and then attached the wheel and weights, beginning with 16 lbs. (if we assume that I had brought the power up to 10 lbs. by hand this would be an additional power of 6 lbs. = 346 lbs. additional pressure) and increasing the weight gradually to power 60 lbs. = 4122 lbs. pressure per square inch. The pressure was thus kept nearly continuous, the snow yielding very slowly to each addition of weight. On adding the last 15 lbs. = 1054 lbs. additional pressure*—the machine was left standing through the night, but the wheel was turned through only about a quarter circle during the whole night This I think is the most perfect ice I have formed as yet, at low temperatures and under slowly applied pressure. It was perfect gray ice uniform throughout, not clouded.

JANUARY 9th, 1871.—Temperatures, 10.15 A. M. −4° F., 12.15 −2° F., 1.15 P. M. −2° F., 2.15 P. M. 0° F., 4 P. M. 0° F., 10 P. M. −4° F. January 10th, 6 A. M. −7° F., 9.30 A. M. 0° F., highest temperature 0° F. Commenced pressure at 10.15 A. M., after having allowed the mould packed with snow to stand for some time. No pressure applied after packing, except by means of wheel and weights over pulley. Commenced with weight of three pounds = about 375 lbs. pressure, and then added two pounds, etc. Discontinued at 9.30 A. M. of the 10th, the thermometer marking 0°. Highest weight used 60 lbs. = 4122 lbs. pressure per square inch. Result perfect, mass changed to ice throughout of remarkably clear and every way fine quality. The weight was increased very gradually, only a few last additions being made in quantities of 6 or 8 lbs. = 400 to 550 lbs. pressure. At this point the motion became very slow. The press was left over night with a weight of 60 lbs. = 4122 lbs. pressure per square inch, and the screw did not probably make more than a quarter turn during the night and up to the time of closing the trial. The snow used was slightly granular.

JANUARY 18th, 1871.—Themometer up to 12 noon has ranged from 16° to 18° F., at 1 P. M. 16° F., 4 P. M. 15° F., 10 P. M. 6° F. January 19th, 7.30 A. M. 0° F., 12 noon 14° F., 4 P. M. 22° F. Highest temperature to which snow was exposed while under pressure was 18° F. The mould was packed with snow last night (the

*The resulting pressure for each additional pound of power diminishes as we go into the higher numbers, owing to increased friction.

17th) the thermometer standing at 16° F.* This morning began with 1 lb. and continued to increase the weight up to 25 lbs. total=1693 lbs. pressure. Discontinued at 11·30 o'clock the 19th. Transformation into ice incomplete. The mass melted partly as ice but the lower portion of the cast from the mould melted more like snow. Notice that less than half the previous weight had been used.

Here are four trials with the iron press, all purposely varied, and all but the last resulting in perfectly formed ice – the ice character in the last instance being developed in the upper portion of the mass, while the lower portion melted more like snow. This test of perfect and imperfect glaciation is worthy of notice. In no way is the true ice character better exhibited than by exposing the mass to a slow melting temperature. If the union of the snow crystals is complete, the mass melts only at the surface, like lake ice, whereas if the conversion into ice is imperfect the mass, as the melting goes on, begins, after a time, to disintegrate and betray a granular structure. Other tests such as whiteness and opacity or their opposites are less reliable. For, though the mass usually clears up under prolonged pressure, the glaciation may be far advanced in a mass which retains much of the whiteness of snow. Nevertheless the usual result is a tolerably clear ice of a grayish appearance owing to the retention of air bubbles. It so happens that the maximum temperature, to which the snow under pressure was exposed, is lowest in the three cases where the resultant ice was most perfect. For these three the highest temperatures are severally 3° F., 15° F., 0° F., and for the last trial 18° F. On the other hand in the second and third of the series, the highest pressure exerted is equal to 4122 lbs. to the square inch as against 1693 lbs. to the square inch in the fourth trial. In the first of the series the power was applied by hand and can not be certainly given, though, as the final power applied is described as "considerable, though by no means violent," it was doubtless much less than 60 lbs. power. It would probably be safe to take it as equal to 45 lbs. which would give an effective pressure of 3068 lbs. to the square inch.

In the first trial I ask the reader further to notice the manner in which the pressure was brought to bear. The screw, which had six threads to the inch, was turned only through a quarter circle at a time, and each quarter was made slowly, so that three hours were consumed before the full pressure of 3068 lbs. to the square inch was brought to bear. As the mould was always packed full of snow, and as the resultant ice usually occupied one-half the depth of a bore $3\frac{1}{2}$ inches deep, this would give a descent of the screw through $1\frac{3}{4}$ inches in three hours, making $10\frac{1}{2}$ revolutions of the screw in 180 min-

* This was done in order to let the whole stand over night without pressure and allow the temperature of the snow and press to become equalized.

utes, or, on an average, a revolution in 18 minutes. But as there was, doubtless, a pause of several minutes between each quarter turn, and perhaps occasional long pauses, the motion of the screw would, while in motion at all, be faster than this, though in any case very slow. It is well to note these features of the trial and to remember that the trial was made in a cast iron mould surrounded by an atmospheric temperature as low as 3° F. Such facts have an important bearing on the possibility of the development of heat under pressure and consequent liquefaction of the snow. In this respect the trial under date, Jan. 9th, 1871, is also instructive. Here a like process of gradual increments was gone through, and here we have additions of power numerically expressed. The weight was increased very gradually (no hand power being used) three pounds to start with equals about 400 lbs. pressure, under which of course the lightly packed snow was yielding; then two pounds, etc., until, only at the last, increments equal to from 400 to 550 lbs. effective pressure were used.

In the fourth trial, beginning was made with only one lb. power, and increase made up to 25 lbs. power.

The times also vary in the four trials, and this fact has its value, though upon that point other and earlier experiments made with wooden moulds are more valuable. For convenience the conditions and results of these four trials may be grouped together as follows:

No.	Date.	High't Temp.	Duration of Exp.	Increments of Effective Pressure.	Resulting Condition.
1	Dec. 29, '70	3° F.	10 hours	Very small by hand, total 3,068 to squ. inch.	Perfect ice.
2	Jan. 4, '71	15° F.	21 hours	Small by hand at first, then 346 lbs. and gradually to total 4,122 lbs. to squ. in.	Most perfect ice.
3	Jan. 9, '71	0° F.	23 hours	375 lbs., 250 lbs., 400 to 550 lbs., total to square inch, 4.122 lbs.	Remarkably clear ice.
4	Jan. 18, '71	18° F.	24 h's +	150 lbs. with additions, to total of 1,693 lbs. to square inch.	Upper part of mass true ice.

From this table and the preceding explanations, the following conclusions are reached:

(1.) Snow may be converted into ice under pressure at a temperature as low as 0° F.

(2.) At a temperature of 3° F. no more than 10 hours are required to complete the process, under a very slowly applied pressure and with gradual increments, until the total reaches probably not more than 3,068 lbs. to the square inch.

It is as well here, as anywhere perhaps, to notice a marked peculiarity of ice and snow under pressure. When a prism of cold ice is placed in the clamps and any given amount of pres-

sure brought to bear upon it by screws, the pressure will not remain constant. The prism will very slowly yield to the pressure, by what seems to be a molecular movement; and if the pressure is to be kept up, it becomes necessary to tighten the screws after a comparatively short time. I regret that the lateral expansion, which may be supposed to attend this longitudinal yielding, was not measured. That such a longitudinal yielding takes place is impressed upon my mind, as a constant experience, and I think there can be no mistake about it. Indeed I find attention called to it in my notes made on the spot, and it will be remarked, that occasional tightening of the screws is mentioned in the prism experiment of Jan. 27, 1869. It is my belief that this molecular yielding of the ice takes place at all temperatures. A parallel fact has certainly been observed, at all temperatures, when snow was being converted into ice. Even with the heaviest pressures used and after the glaciation has been completed, the mass still continues to yield under the pressure. Here in a firmly bound iron mould, there can be no lateral expansion, but the ice particles seem to approach each other and compact themselves, not to expand again when the pressure is removed. It is impossible to keep the screw tight without a weight movable over a pulley. If the screw be tightened by hand and left for an hour or two, the ice contracts under it and leaves room so that the next slight turn of the screw is comparatively easy.

Having thus established the main fact of the glaciation of snow of low temperature under pressure, we come to the discussion of the question whether, under the circumstances of these experiments, we are to suppose that glaciation is due to the production of water, or at least of moisture, under the pressure applied.

For this purpose it will be necessary to combine with the observations above given, some earlier ones, made with wooden moulds, fitted with wooden plungers. It will be understood that trials next to be mentioned were of that kind. It is not necessary to discuss in this connection the rule of the brothers Thomson, mentioned above. The considerations advanced in regard to the prisms apply equally to the glaciation of snow. The temperatures involved are far out of the way of that law of liquefaction. I purpose, rather, to lay before the reader the actual facts of observation, gathered from numerous efforts to determine the question.

The question of liquefaction at low temperature, according to the law of the brothers Thompson, being disposed of, another question arises: whether in the mass of snow under these larger pressures sensible heat can by any possibility be developed sufficient to account for liquefaction. In the outset such a

supposition seems wholly unwarranted. I have only to refer to the production of ice in the iron press, with its slow and intermitted motion, in order to call to mind the extreme improbability of a rise of temperature from near the zero point to the point necessary for liquefaction under the pressures used. Moreover, I have endeavored by actual trials to obtain the extreme effect of such pressures as have been used—pressures not so great indeed as the highest of those used with the iron press—but great enough to convert snow into ice at very low temperatures.

The most reliable of these observations have been made upon not very cold snow with pressures rising from 580 lbs. to the square inch, to a possible maximum of 2,000 lbs. to the square inch. I shall hereafter give cases of the glaciation of snow of low temperature under the latter pressure. The method of testing has been as follows: The temperature of the air, and the snow before pressure is noted, and in some cases, also, the temperature of the moulds. Then pressure is rapidly applied to the snow in the moulds by sudden impact of the screw. A wire in the bottom of the plunger leaves a fine bore in the pressed snow for the insertion of a thermometer, and the temperature of the snow is noted immediately on withdrawal of the plunger. In some instances pains have been taken to aggravate the rise of temperature by sudden and violent impact; throwing myself on to the handles of the screw with all my might. Now the highest elevation I have been able to obtain by this violent method of applying a pressure of 1,500 to 2,000 lbs. to the square inch, during only one minute, was $3\frac{3}{4}°$ F. The pressure exerted was my whole strength applied to the handles of the screw by sudden and violent efforts. Unfortunately the temperature of the moulds before pressure was not observed.

In the very next observation recorded, I find the same operation gone through with, except that the violent impulses were repeated, at intervals, during twenty minutes. The pressure was the same as before, but the elevation effected was only $1\frac{1}{4}°$ F. Here also the temperature of the moulds was not observed. I have before me another instance where the same pressure was applied, only during two minutes, to a mould filled with bits of ice. The temperature of the air and of the moulds before pressure are noted at 27° F. The ice before pressure measures 25° F., and after pressure 27° F. The elevation in this case was doubtless aided by that of the moulds, and in the case before given, when the temperature after pressure rose to $3\frac{3}{4}°$ F., it is very probable that the temperature of the moulds was higher than that of the snow; while in the second case, there is reason to think it was lower than that of

the snow before pressure. In this way the $3\frac{3}{4}°$ F. would be too much and the $1\frac{1}{2}°$ F. would be too little. From numerous observations, I am well satisfied that the effect of pressure *quietly applied*, would not exceed a degree, or a degree and one-half of Fahrenheit; and where *special pains are taken to avoid elevation of temperature*, its effect will be so diminished that they may be left out of the account, in experiments conducted a number of degrees below freezing. If it is said that the method of taking the temperatures is crude, this must certainly be admitted. But it is the best that could be devised. I have indeed attached a thin, hollow pointer of iron to the plunger, in the center of which I carried a thermometer down into the snow and strove by this means, to get at the temperature during pressure. But after numerous trials, the indications proved to be so unreliable that I have discarded the results. It may be said, however, in favor of the proximate accuracy of the temperatures quickly taken after the pressure is removed, that, as the snow is not elastic, the heat evolved by pressure will not be absorbed again on its withdrawal.

So far as the observations went they were entirely confirmatory of the above conclusion. The thermometric evidence being against the supposition of any development of moisture by heat, I so arranged matters that I could actually watch the the progress of glaciation, while pressure was being applied. Wooden moulds were prepared with glass windows of about an inch in diameter in the opposite sides. These moulds were as usual in halves, bolted, so that the resulting ice could be exposed. Through the windows, which were of very heavy plate glass, I watched the progress of glaciation from beginning to end. But the closest scrutiny failed to disclose any signs of moisture. Of any free and abundant flow of water through the snow such as is seen at a temperature of 32° F., it will be understood we are not here discoursing at all. Any such thing would have been readily detected without nice contrivances. We are considering only the production of a proper moisture, which might be forced through the mass so as to wholly permeate it. Of such moisture I found no trace in the lower temperature experiments. When the pressure had been well brought to bear, a change could be seen coming over the mass, as will be detailed in a trial given below. The snow in the mould, as seen through the glass, began to have a glazed appearance, and soon seemed to be losing its whiteness and opacity. This change went on gradually until the whiteness of the snow was nearly gone. I could discern upon the glass no trace of condensation of moisture, no clouding as if by a breath, though the temperature of the room was only a few degrees above zero of Fahrenheit.

Another attempt was made to detect the presence of moisture. I sprinkled into the snow little particles of water-paint. These paints are quite sensitive to moisture; so much so that, if buried in lightly packed snow without pressure, at a temperature as low as 12° F., they attract moisture and soften by an action similar to that of common salt. And when barely touched to really moist snow of 32° F., the snow is promptly discolored with a blotch which spreads around the point of contact.

Now it is manifest that this paint furnishes a delicate test of the presence of moisture. If at temperatures lying in the region of zero Fahrenheit moisture were being forced through the interstices of the snow, even in minute quantity, it would necessarily carry with it some traces of color from the paint sprinkled in the mass; and this is observed to be the case with snow under pressure near to 32°, the paint spreading in large blotches. At a temperature as low as 20°, the paint seems to liquefy the snow to such an extent, that if it be introduced with the snow into the moulds, it will become quite moist and will perhaps form a blotch. I infer that the blotching at this temperature is due to the liquefying action of the snow, because as will be seen in the trials detailed below, no sign of such spreading could be detected at the temperature under which glaciation was there effected. It must be regarded as certain therefore, that if moisture is developed at any time during the process, it is not developed until the snow has been so completely consolidated as to prevent the passage among its particles of even so minute a quantity of moisture as would be necessary to carry color along with it. But snow so consolidated would be little if any short of ice. I have thought it better to state the points involved in the following observations before giving their details.

Observations with wooden moulds.

Experiment No. 1, February 7th.—Temperatures. 12 noon 9° F., 4.20 P. M. 3° F., 9.50 P. M. —7° F., 7.30 A. M. February 8th, —21° F., highest temperature 9° F.

The screw was gradually brought to bear with considerable power, no pains being taken to avoid accumulating heat. In about two hours a change was manifest in the snow. It began to have a glazed appearance and soon seemed to be losing its whiteness and opacity. This change went on gradually, until, at about 10 P. M., the whiteness of the snow, as seen through the glass, was nearly gone. It was allowed thus to stand over night. At 9.30 A. M. the pressure was taken off. The ice was perfect. The whole prism had lost its opacity so far as to transmit the light, the snowy whiteness was gone and the whiteness of ice—a gray ice had taken its place. A layer of particles of water paint was in full view, on the top of the resulting prism (the bore of the wooden moulds was square), just under the surface. There was not a sign of the color having spread. Their angles were perfectly sharp and their lines perfectly well defined. The same was true when the prism was opened. The particles scattered through the mass exhibited no sign of spreading.

Experiment No. 2.—Snow used marked on the ground 5° F. Temperatures, 9.45 A. M. of February 10th 3° F.—during the day near 0° F. 10 P. M. −6° F., 9 A. M. of February 11th −5° F., 10 P. M. 4° F., 7.30 A. M. of February 12th 6° F., 8.30 A. M. 7° F. Duration of trial from February 10th 9·45 A. M. to February 12th 8.30 A. M., making 47 hours nearly.

Used here a pair of deep moulds, packed full of snow, which accounts for the imperfect glaciation of the lower portion. The bottom was granular snow. Then came vitreous snow. These two occupied one-third the length of the prism. Then came ice of a grayish appearance, increasing in perfection to the well defined ice of the upper portion. The ice is whitish or turbid but well characterized. The water paint in the prism shows not a trace of running. The angles are sharp and the lines sharply defined. On the line between the snow and ice two particles of paint were lodged, but here there had been no change in their appearance.

Experiment No. 3.—Temperature of air February 22d, 1868, at 9 A. M. −4° F. Temperature of moulds −4°. Temperature of snow in moulds before pressure −4°. Snow used was granular.

Temperatures, 9 A. M. −4° F., 12.15 P. M 0° F., 2 P. M. 0° F., 4 P. M. 1° F., 7 P. M. −2° F., 9.40 P. M. −7° F., 8 A. M. of February 23 −5° F., 1 P. M. 0° F., 9.30 P. M. −5° F. February 24th, 7.30 A. M. −10° F., 9 A. M. −7° F. Duration 48 hours. Opened and found a gradual transition from granular snow at the bottom of the prism to perfect ice at the top. The ice was clear and fine. The paint used in this experiment was very finely divided, so that the particles might be affected by the least moisture, and yet in the best of the ice there was no trace of a spreading of the color. Each particle was a distinct speck in the ice.

In looking over the various observations upon snow, in connection with those made upon prisms of ice held in clamps, everything seems to negative the supposition that, at the low temperatures used, moisture intervenes to prepare the way for regelation. In the case of prisms fully exposed to view, there is no sign of such a process. Also in the case of prisms, and snow as well, it has been shown that the rule for the depression of the point of liquefaction cannot apply under such pressures as have here been used. Neither is there good reason to suppose that the development of sensible heat is sufficient to have an important bearing on the results. Moreover, inasmuch as regelation is of necessity an instantaneous process, the evidence that time plays a not unimportant part in the process by which continuity is established between fragments of ice as well as snow particles, though that evidence hereafter to be reviewed is not as definite or complete as could be wished, bears, so far as it is allowed weight, against the application of that theory to the facts here insisted on.

I would not however be supposed to overlook possibilities which must be contemplated in this connection, though they seem remote. It may be conceived, that, in the case of prisms and of snow packed in moulds, inasmuch as it is not possible to bring the pressure to bear evenly, either upon all points of fractured faces of prisms, or upon all points of the mass of snow, there might be now here and next there a concentration of pressure locally upon molecules, in consequence of which, now at this point and next at another, slight moisture might be developed, in which case true regelation would take place. In

this form my friend Mr. Willson has suggested to me a possibility, akin to one which I myself had considered, namely that of local leverage among the particles, by which large local pressures might be concentrated here and there in the mass. But it must be remembered that the paint test demonstrates that there can be no speading of moisture from such centers; and as a further counter-suggestion, Mr. Willson points me to the fact that we should, in this way, secure only partial re-union, at single points along the faces of fracture in prisms, and only local glaciation of the snow at scattered centers, instead of the uniform glaciation of the whole mass, which results when the pressure is prolonged.

It remains only to lay before the reader such evidence as the facts give us of the influence of time upon the processes we are discussing.

I think it will serve our purpose best, at this point, to introduce a brief tabular exhibit of quite a number of trials, arranged in the order of temperatures; premising that an unavoidable indefiniteness attaches to the contained results. Those pressures which had to be estimated are given within maximum range only. I think I am perfectly safe however, in saying that I have given an exaggerated extreme upper limit. Besides this it is to be noted that pressures exerted in this way, upon a yielding mass of snow in a mould, do not tell upon the lower portion of the mould, until the snow has had time to settle away and the pressure is followed up. So that full effect is at first experienced only near the face of the plunger, and then only for a short time; after which, the yielding of the mass relieves the pressure. This introduces an element of uncertainty—by bringing the amount of pressure for a great deal of the time, and for a large portion of the mass, below the figures taken to express it. It is evident, too, from a glance at the table, that we can not state the minimum of time and pressure required at any given temperature for glaciation. All we can do is to say, that, at particular temperatures, and with certain pressures, no more than a certain length of time is required. We have only some evidence regarding minimums. It will be noticed also that here ice is not considered perfect, unless the snowy whiteness has been in a good degree replaced by a darker shade, more approaching that of lake ice, and unless there has been an elimination of the granular structure.

Tabular Exhibit of Observations on Snow.

No.	Temp. F.	Time.	Pressure to square inch.	Result.
1	32°	1 m.	242	Gray ice, imperfect.
2	32	2·5 m.	950–1275	Sub-vitreous, sub-granular, gray ice.
3	32	3 m.	532	Gray ice, imperfect.
4	29	3 m.	950–1275	Snow and coarse granular, very good ice.
5	27·5	1 m.	580	Hard but quite granular.
6	26	1 m.	1475–1975	Sub-vitreous, sub-granular.
7	26	24 h.	950–1275	Very perfect, pretty clear ice.
8	25	1½ h.	1475–1975	Very perfect ice.
9	24·5	10 m.	580	Sub-vitreous, granular.
10	23·25	20 m.	1475–1975	Sub-vitreous, grayish, sub-granular.
11	23	20 m.	1100–1465	In five minutes snow-darkened, glaciation surely complete.
12	22	3 h.	950–1275	Vitreous, dark, translucent ice character well advanced.
13	21	45 m.	950–1275	Good ice, rather white.
14	20	2 m.	950–1275	Sub-vitreous, opaque, granular, pure white.
15	20	10 m.	575– 775	Sub-vitreous, granular.
16	20	3 h.	1200–1600	Good ice.
17	18	24 h.	1693	True ice but not as perfect as some.
18	15	21 h.	4122	Perfect gray ice uniform throughout.
19	9	22 h.	1475–1975	Change manifest in two hours and in ten hours change was far advanced, perfect gray ice.
20	7	47 h.	1475–1975	Perfect ice.
21	4	5 h.	1475–1975	Highly vitreous, dark, melts with only slightly granular structure.
22	3	10 h.	3068	Perfect ice.
23	1	48 h.	1475–1975	Perfect ice.
24	0	23 h.	4122	Remarkably clear fine ice.
25	−2	2 h.	1475–1975	Highly vitreous, further advanced than No. 21.

If, with the above explanations in view, we direct our attention to special portions of the table, we find, in the first place, that at 32° F., with a very moderate pressure, a gray ice may be obtained in from one to three minutes; and no marked difference is observed between ice produced under an extreme pressure of 250 pounds to the square inch, and that produced under higher pressures.

No. 6, with temperature of 26°, subjects snow to a pressure of from 1475 pounds to 1975 pounds to the square inch for one minute. This high pressure, favored by a not very low temperature, fails in so short a time to produce perfect ice, while in No. 7, at the same temperature, under a lower pressure, we get in 24 hours "very perfect, pretty clear ice."

At 25° F. No. 8 gives us very perfect ice in an hour and a half under a pressure of from 1475 pounds to 1975 pounds per square inch; and still better is No. 11 in which, at a temperature of 23°, and under a pressure ranging from 1100 pounds to 1465 pounds to the square inch, we get a nearly complete gla-

ciation in twenty minutes, with some darkening of snow in five minutes.

In the next five trials, from No. 12 to No. 16 inclusive, we have a range of only two degrees of temperature, and such a relation of times and pressures as will throw, perhaps, some light on the point under discussion. We will arrange these five trials in order, according to the perfection of the ice:

No. 15	20° F.	10 m.	575 to 775 lbs.	Sub-vitreous, granular.
No. 14	20° F.	2 m.	950 to 1275 lbs.	Sub-vitreous, opaque. Granular, pure white.
No. 13	21° F.	45 m.	950 to 1275 lbs.	Good ice; rather white.
No. 12	22° F.	3 hours	950 to 1275 lbs.	Vitreous, dark, translucent. Ice-character well advanced.
No. 16	20° F.	3 hours	1200 to 1600 lbs.	Good ice.

The pressures in Nos. 14, 13, and 12 are the same, and the temperatures so near together as to point pretty distinctly to the influence of time.

Passing over the higher pressures and long times in the lower part of the table, I call attention to Nos. 21 and 25. There is an apparent discrepancy between these two, which is probably due to a heavier pressure, within the range set down, being brought to bear upon No. 25. These two trials show, however, that even in a couple of hours at a temperature as low as −2° F., under a pressure certainly less than 2,000 pounds to the square inch, glaciation may be very far advanced; but it is still to be noticed that the terms expressing the result are much more positive in No. 23, for instance, where, with only three degrees difference in temperature, the long continued pressure of like range gives perfect ice; and it will be noticed that when we come among the lower temperatures such strong expressions are only used where the pressure has been long continued. These facts taken with those of like nature observed when experimenting upon prisms; taken also with a general experience running in that direction, incline me to the conclusion that time is a very important factor in the process of glaciation. Impressions early made by observations have grown into a strong conviction that glaciation would take place in a few days under the moderate pressure of say 1,000 pounds to the square inch, allowed to bear quietly and continuously on snow the temperature of which did not exceed that of zero, Fahrenheit.

The results of these observations may now be briefly summed up. We may regard as established—

(1.) That broken prisms of ice, at temperatures far below 32° F., will firmly reunite if the faces of fracture are brought together and a very moderate but long continued pressure is applied.

(2.) That snow is converted into ice under a long continued pressure, not exceeding 2,000 pounds per square inch, and probably much less than that, and at temperatures near to zero Fahrenheit.

(3.) That while great pains have been taken to detect the presence of moisture in the snow while under pressure at these temperatures, no satisfactory evidence of its presence has been obtained, the various tests failing to give it; while, on the other hand, the possibility must be entertained of the local development of moisture in centers of concentrated pressure; though the paint trials demonstrate that, if produced, it does not flow from the centers, and so a serious difficulty arises as we contemplate this possibility as influencing the result.

(4.) That the evidence is not inconsiderable in favor of the influence of time as a factor in the establishment of continuity between fragments of ice and between snow crystals and, insofar as this consideration is admitted, it militates against the supposition of liquefaction and regelation at these temperatures.

ART. LII.—*Communications from the U. S. Geological Survey, Rocky Mountain Division.* I. *On the minerals, mainly Zeolites, occurring in the basalt of Table Mountain, near Golden, Colorado;* by WHITMAN CROSS and W. F. HILLEBRAND.

[THE communications, of which the following is the first, are intended as preliminary notices of interesting facts, in cases where the publication of the report which would embrace them is necessarily delayed for a considerable length of time.

In the course of an investigation, now being carried on in this division, into the geology of a portion of the eastern flanks of the Colorado Range near the town of Golden, the opportunity presented itself of making a study of the interesting basaltic flows at that point, and the minerals contained therein. The present contribution by Messrs. Cross and Hillebrand gives a brief description of the species thus far fully determined, viz: chabazite, thomsonite and analcite. The latter mineral has hitherto been supposed by many persons who have collected it at this point, basing their assumption on statements made in the Hayden Reports,* to be leucite, which is believed to be unknown elsewhere in this country, except as a microscopical constituent of certain rocks, collected by me in Wyoming, when on the Fortieth Parallel Survey. Since the Colorado leucite proves to be analcite, the Leucite Hills of Wyoming seem to be still the only known locality of leucite in the United States.

S. F. EMMONS, *Geologist in charge.*]

* Annual Report of the U. S. Geol. and Geog. Survey of the Territories for 1873—pp. 130 and 359.

Table Mountain is situated upon the plain, twelve miles west of Denver, although it approaches so near to the abruptly rising foot-hills, composed of Archaean schists, that the intervening space is scarcely more than a mile in width. Within this space, however, are exposed the upturned edges of strata of the Trias, Jura, and Cretaceous, including the coal-bearing horizon of the Laramie formation; dipping very steeply east.

Table Mountain owes its existence to a sheet, or rather in most parts, to two sheets of basalt, which have in a measure protected the underlying strata from erosion. Through that agency the upturned strata adjacent have been denuded until a valley several hundred feet deep has resulted, separating Table Mountain from the foot-hills.

The horizontal strata of Table Mountain seem at first sight markedly unconformable with those so near by, but this appearance is due to the location of this valley of erosion directly in the sharp fold. In other places the conformability is distinctly shown.

Opposite the mountain, Clear Creek issues from the foot-hills and has cut a gorge directly through it, forming what are known as North and South Table Mountains. On the banks of the creek, behind the mountain, is situated the town of Golden.

The mountain is a plateau or *mesa* of not more than four to five square miles in area, with an average elevation above the bed of Clear Creek of about 700 feet.

The source of the basalt is in certain dykes situated to the north and northwest, whence the lava spread out in a sheet and flowed to the south-southeast. The lower sheet averages one hundred to one hundred and fifteen feet in thickness, and possesses the structure of a stream which has flowed upon the surface. Adjacent to the underlying strata, is a layer of a very porous structure, about one foot in thickness; then follows massive compact rock for seventy feet, while the upper forty to fifty feet show many cavities, large and small, more or less flattened and drawn out, and the surface is rough and jagged. The first sheet was covered by a second, of identical composition, before erosion or decomposition had produced noticeable effects. Of the second sheet, only the massive lower part remains, and its thickness, as well as the lateral extension of both sheets, can only be a matter of conjecture. The rock is a feldspar basalt of very simple composition; rather coarsely crystalline in its massive parts, and in the porous with a groundmass which becomes finer-grained as it approaches the contact or the surface. A glassy base, now much devitrified was formerly present in the porous parts.

In the cavities of the upper portion of the lower sheet are

many beautifully crystallized zeolites, associated with calcite and aragonite. The mode of occurrence varies somewhat in different parts of the mountain, and a few species seem to be locally restricted, but most of those to be described can be found in abundance and well developed.

At a point on the southern face of North Table Mountain, where the greatest number of species occur together, the students in the school of mines at Golden began blasting for specimens several years ago. During the present investigations the locality was still further opened, and absolutely fresh and clear material obtained. Up to the present time, the following species have been definitely determined, viz: analcite, apophyllite, aragonite, calcite, chabazite, mesolite, natrolite, stilbite and thomsonite. Several others are in process of investigation, some of which will doubtless prove to be modifications of some of the species mentioned. In the further description, the order to be followed is primarily that of deposition at the above locality, where frequently as many as six or seven species occur together.

The only previous notice of this mountain is embodied in the report of Arch. R. Marvine, in Annual Report of the U. S. Geological and Geographical Survey of the Territories for the year 1873, pp. 129–131, but the information given is exceedingly meager, and in part entirely incorrect.

The Survey is indebted to Professor A. Lakes, of the School of Mines, for calling special attention to Table Mountain.

1. *Chabazite.*

This mineral seems to be the oldest of the zeolites, with the exception of certain peculiar stratified deposits in some of the cavities, the character of which has not been fully determined. It was however preceded in some places by yellow calcite, as will be described later.

In most cases where several zeolites occur together, chabazite forms a coating to the cavity, and upon it the others are deposited. It frequently occurs alone however and excellent specimens are abundant. The glassy, white, or rarely pinkish crystals have the usual rhombohedral form, and the faces are occasionally as much as 1 cm. in diameter. The crystals are usually twins of interpenetration.

In some of the smaller cavities are sub-dividing walls of chabazite, or miniature columns composed of many small crystals.

A second generation of chabazite came after thomsonite and analcite, but the crystals are few and very minute. No optical or chemical examination has as yet been made.

2. *Thomsonite.*

This zeolite followed closely upon the chabazite, indeed its earliest aggregates are so intimately associated with that mineral, as to make it evident that the deposition of the one had begun before that of the other had ended. The mineral occurs in very minute rectangular blades, which are placed upon each other like the leaves of a closed fan, and the very compact combinations of such aggregates are usually arranged in a more or less distinctly radiate manner. Sometimes spherical forms result; in other cases, columns, by radiation from an axis; or less frequently, walls, the blades standing at right angles to the central plane. Where crystals of yellow calcite were not covered by chabazite, the thomsonite has never failed to coat them, the blades being approximately perpendicular to the crystal faces. When a large surface of chabazite has been completely coated by the more or less radiate aggregates of thomsonite, forming an undulating surface, the whole has a most delicate silken lustre, while that on a fractured surface of a spherical mass is more satin-like. The aggregates are white when pure; single blades are transparent.

It is difficult to obtain isolated leaves, and they are then so thin as to affect polarized light but slightly; still, it was definitely determined that the direction of total extinction between crossed Nicols is parallel to the longitudinal axis. By examination of the ends of the blades, under the microscope, with a power of ninety diameters, it could be seen that the vertical edges were formed in many cases by a plane at right angles to the dominant surface; occasionally a prism with angle very near 90°, and in a few cases both forms together were seen. These planes correspond to the macropinacoid (dominant), prism and brachypinacoid of thomsonite as given by all authorities. The termination seems to be the basal plane. The average thickness of these blades is about 0·01^{mm}. Toward the close of the zeolitic formation, a second generation of thomsonite was deposited. The blades are in this case longer than those of the first, while the other dimensions remain about the same.

In combination with the basis upon the crystals of this latter growth are apparent brachydomes, whose angles with the basis were most frequently measured at about 145° or 135°.

The blades of the second generation, when deposited upon those of the first, have the same crystallographic orientation, and serve simply as prolongations of the latter. Through the usual slight yellowish tinge of the recent growth, the line between the two is clear.

The long blades of the second growth of thomsonite often form bundles resembling rough prismatic crystals, and these

bundles arranged in a loosely and irregularly radiate manner form bunches an inch or more in diameter, upon which the delicate needles of mesolite have a special tendency to deposit themselves. Reference will be made to this again later.

Of the following chemical analyses, that under I is of the older, and that under II of the more recent growth. In each case the percentages under *b* denote repeated determinations. The material for I was carefully selected with a view to purity. The imperfectly spherical aggregates were broken up quite fine, and each particle appropriated for analysis was first closely examined with the loupe, in order to detect any trace of chabazite. Analysis II was made of material taken from one of the bunches above described. The microscope showed that minute hairs of mesolite were so intimately associated with the thomsonite as to render their *complete* removal impossible. The specimen chosen had comparatively few of them, and the increased percentage of SiO_2 probably indicates the amount of the impurity present.

	I.		II.		III.	IV.
	a.	*b.*	*a.*	*b.*		
SiO_2	40·681	40·703	42·662		41·6	40·877
Al_2O_3	30·117	29·749	29·252	29·286		
CaO	11·921	11·895	10·900	10·900		
Na_2O	4·444		4·920			
H_2O	12·857		12·275	12,337		
	100·020		100·009			

From the above figures, the following oxygen ratios are obtained:

	RO	:	Al_2O_3	:	SiO_2	:	H_2O
I.	1	:	3·09	:	4·76	:	2·51
II.	1	:	3·10	:	5·17	:	2·48

These ratios agree quite well with those in the variety formerly called mesole or faröelite. But that thomsonite which seems to be considered by Dana, Rammelsberg, Naumann-Zirkel and others, has a ratio of about $1 : 3 : 4 : 2\frac{1}{2}$, and Dana* directly intimates that the high percentage of silica in mesole, is due to an admixture of quartz, mesolite, etc. Thinking it barely possible that some chabazite had escaped notice in obtaining material for I, thus increasing the percentage of silica, a second portion of thomsonite from the same specimen was procured, by simply breaking off the *ends* of the blades, and this was found to contain 41·6 per cent. SiO_2 (III). A microscopic examination of this material showed that these blades

* A System of Mineralogy, ed. of 1874. p. 424.

of thomsonite contained very small, irregularly rounded particles, imbedded in the outer surfaces. These particles which are clear, or slightly yellowish, do not affect polarized light perceptibly, and are not crystalline in form. They are present in varying quantity toward the termination of nearly all blades, but are usually entirely wanting in the lower portions of all aggregates.

Analysis IV was made from material taken from the purest and finest specimen of thomsonite as yet obtained. Here the deposit of thomsonite was unusually thick, and the separation from the underlying thin layer of chabazite was complete. The particles above referred to were present in very small quantity in the upper part of the blades, and even if they consisted of pure silica could have affected the result but slightly.

While believing that the results obtained indicate a greater variation in the composition of thomsonite, than is allowable under the generally accepted formula of Rammelsberg,*

$$\left\{ \begin{matrix} m\,(2CaAlSi_2O_8+5aq.) \\ n\,(2Na_2AlSi_2O_8+5aq.) \end{matrix} \right\},$$

the further discussion of the question is reserved for the final report.

It was found that a varying quantity of water (usually about 2 per cent) could only be driven off at a very high temperature, but as yet, no simple molecular ratio between this water and the compound has been found. The material was dried at 100° C. The action of this thomsonite before the blowpipe is entirely normal.

The thomsonite of Table Mountain was first identified, through a quantitative analysis, by Carlton H. Hand, at the time a student, now, assistant in assaying, in the School of Mines, at Golden. The analysis was never published, and has since been lost.

3. *Analcite.*

Analcite follows thomsonite in time of deposition. At the locality mentioned on North Table Mountain, its crystals are pure white or transparent, and vary in size from very small ones, to those nearly an inch in diameter. The predominating form is the common trapezohedron 2-2 (202), having its octahedral edges evenly truncated by $\frac{3}{2}$ ($\frac{3}{2}$O). The appearance of $\frac{3}{2}$ is very characteristic of the Table Mountain analcite, though the form is never prominent, and is usually represented by an exceedingly narrow line. According to Zirkel,† the form $\frac{3}{2}$ has been observed but once on analcite, viz: by Laspeyres on that from the Kerguelen islands.

* C. Rammelsberg, Mineral Chemie, 1875, p. 657.

† Naumann-Zirkel, "Elemente der Mineralogie." Leipzig, 1881.

All of this analcite examined is doubly refractive, but so irregularly that it cannot well be used in confirmation of the interesting observations of Ben-Saude,* according to whom the optical anomalies of analcite are only to be explained by the theory of tension (Spannung) within the crystal, caused by some irregularities of growth.

A second generation of analcite was observed, analogous to that of chabazite, the crystals being very small and clear. They were observed upon apophyllite.

On the eastern side of North Table Mountain, analcite is often present alone, filling small cavities completely, and developed in immense crystals in the large ones. Some crystals here were nearly two inches in diameter. The faces of such large crystals are often uneven through depressions or other irregularities bounded by faces of 2-2 (202).

On South Table Mountain analcite is more abundant than any other zeolite. Large cavities have a floor of analcite, but always in quite small crystals. Natrolite is almost invariably deposited upon it, and frequently small cracks are filled with the same mineral, associated with calcite or natrolite.

Analcite was also found in spaces between the pebbles of a conglomerate bed, not far below the basalt, the pebbles being of basic eruptive rocks.

Denver, Colorado, April, 1882.

[To be continued.]

ART. LIII.—*On a new Locality for Hayesine and its novel occurrence;* by NELSON H. DARTON.

HAYESINE has only been found heretofore, as far as I can find, at Iquique, South America, upon the western coast of Africa, and in Nova Scotia, and occurring with gypsum and some allied species, the variety generally containing soda in greater or less but no fixed proportion. I now add another locality, which is Bergen Hill. It occurs here, not with gypsum, etc., but with datholite and calcite in cavities in the trap rock. At the new railroad tunnel now in progress near Weehawken, New Jersey, when down in shaft No. 2 of the tunnel at a distance of about 1600 feet from the east face of the dike and 185 feet below the ground above, I found in examining a vein of calcite, etc., exposed on the north side, a peculiar mineral lying upon the crystals in the geode that I did not recognize for this locality. It evidently was not pectolite as the fibers were quite soft, even pulverulent. They seemed more

* Alfredo Ben-Saude, "Ueber den Analcim," in "Neues Jahrbuch für Mineralogie" etc., 1882, I. 41.

like asbestos than any other species, but still in doubt I secured all that was possible, and when arriving in New York made a chemical and physical examination of it. A preliminary blowpipe treatment showed it to be readily fusible and when moistened with acid the characteristic green flame proved it to be a borate. The analysis yielded the following results:

		Calculated to $CaB_2O_7 + 3H_2O$.
Lime	18·39	18·42
Boracic acid	46·10	46·05
Water	35·46	35·53
Silica, Soda, Magnesia	trace	
	99·95	100·00

The traces of silica and magnesia were probably due to exterior causes, but the presence of the soda was very marked. The specific gravity was between 1·5 and 1·7. I could find but 7·3 decigrams, and as the greater part of this has been used in the examinations I have but 2·6 decigrams left; but I have reason to believe that much more may be procured by cutting into the vein for it, and besides there may be many reproductions of the same conditions under which it occurred. The form was that of acicular crystals about 4^{mm} long and very slender. They were grouped together in the form of little mats and these lay loosely upon the crystals of calcite (dog-tooth spar). These geodes were surrounded by others of datholite and enclosed in a vertical vein 2½ to 4 inches wide, partly filled with the primary forms of calcite and chlorite. The rock is quite soft and very permeable to infiltering water. Thus some borate of lime has been taken into solution under peculiar and unknown conditions and crystallized after running into the geode which held it. All these geodes are open at the top, this is directed toward the upper side of the vein, and would thus arrest and hold infiltering solutions. I intend to investigate how datholite is decomposed and borate of lime separated out of its constituents.

Laboratory, 319 Pearl street.

ART. LIV.—*Notes on the Electromagnetic Theory of Light;* by J. WILLARD GIBBS. NO. II.—*On Double Refraction in perfectly transparent Media which exhibit the Phenomena of Circular Polarization.*

1. IN the April number of this Journal,* the velocity of propagation of a system of plane waves of light, regarded as oscillating electrical fluxes, was discussed with such a degree of approximation as would account for the dispersion of colors and give Fresnel's laws of double refraction. It is the object of this paper to supplement that discussion by carrying the approximation so much further as is necessary in order to embrace the phenomena of circularly polarizing media.

2. If we imagine all the velocities in any progressive system of plane waves to be reversed at a given instant without affecting the displacements, and the system of wave-motion thus obtained to be superposed upon the original system, we obtain a system of stationary waves having the same wave-length and period of oscillation as the original progressive system. If we then reduce the magnitude of the displacements in the uniform ratio of two to one, they will be identical, at an instant of maximum displacement, with those of the original system at the same instant.

Following the same method as in the paper cited, let us especially consider the system of stationary waves, and divide the whole displacement into the *regular* part, represented by ξ, η, ζ, and the *irregular* part, represented by ξ', η', ζ', in accordance with the definitions of § 2 of that paper.

3. The *regular* part of the displacement is subject to the equations of wave-motion, which may be written (in the most general case of plane stationary waves)

$$\left.\begin{aligned} \xi &= \left(\alpha_1 \cos 2\pi\frac{u}{l} + \alpha_2 \sin 2\pi\frac{u}{l}\right)\cos 2\pi\frac{t}{p}, \\ \eta &= \left(\beta_1 \cos 2\pi\frac{u}{l} + \beta_2 \sin 2\pi\frac{u}{l}\right)\cos 2\pi\frac{t}{p}, \\ \zeta &= \left(\gamma_1 \cos 2\pi\frac{u}{l} + \gamma_2 \sin 2\pi\frac{u}{l}\right)\cos 2\pi\frac{t}{p}, \end{aligned}\right\} \qquad (1)$$

where l denotes the wave-length, p the period of oscillation, u the distance of the point considered from the wave-plane passing through the origin, α_1, β_1, γ_1 the amplitudes of the displacements ξ, η, ζ in the wave-plane passing through the origin, and α_2, β_2, γ_2 their amplitudes in a wave-plane one-quarter of a

* See page 262 of this volume.

wave-length distant and on the side toward which u increases. If we also write L, M, N for the direction-cosines of the wave-normal drawn in the direction in which u increases, we shall have the following necessary relations:

$$\mathrm{L}^2+\mathrm{M}^2+\mathrm{N}^2=1, \tag{2}$$

$$u=\mathrm{L}x+\mathrm{M}y+\mathrm{N}z, \tag{3}$$

$$\mathrm{L}\alpha_1+\mathrm{M}\beta_1+\mathrm{N}\gamma_1=0, \quad \mathrm{L}\alpha_2+\mathrm{M}\beta_2+\mathrm{N}\gamma_2=0. \tag{4}$$

4. That the *irregular* part of the displacement (ξ', η', ζ') at any given point is a simple harmonic function of the time, having the same period and phase as the regular part of the displacement (ξ, η, ζ), may be proved by the single principle of superposition of motions, and is therefore to be regarded as exact in a discussion of this kind. But the further conclusion of the preceding paper (§ 4), "that the values of ξ', η', ζ' at any given point in the medium are capable of expression as linear functions of ξ, η, ζ in a manner which shall be independent of the time and of the orientation of the wave-planes and the distance of a nodal plane from the point considered, so long as the period of oscillation remains the same," is evidently only approximative, although a very close approximation. A very much closer approximation may be obtained, if we regard ξ', η', ζ', at any given point of the medium and for light of a given period, as linear functions of ξ, η, ζ and the nine differential coëfficients

$$\frac{d\xi}{dx}, \quad \frac{d\eta}{dx}, \quad \frac{d\zeta}{dx}, \quad \frac{d\xi}{dy}, \quad \text{etc.}$$

We shall write ξ, η, ζ *and diff. coëff.* to denote these twelve quantities.

From this it follows immediately that with the same degree of approximation $\dot{\xi}'$, $\dot{\eta}'$, $\dot{\zeta}'$ may be regarded, for a given point of the medium and light of a given period, as linear functions of $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$ and the differential coëfficients of $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$ with respect to the coördinates. For these twelve quantities we shall write $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$ *and diff. coëff.*

5. Let us now proceed to equate the statical energy of the medium at an instant of no velocity with its kinetic energy at an instant of no displacement. It will be convenient to estimate each of these quantities for a unit of volume.

6. The statical energy of an infinitesimal element of volume may be represented by $\sigma\,dv$, where σ is a quadratic function of the components of displacement $\xi+\xi'$, $\eta+\eta'$, $\zeta+\zeta'$. Since for

that element of volume ξ', η', ζ' may be regarded as linear functions of ξ, η, ζ *and diff. coëff.*, we may regard σ as a quadratic function of ξ, η, ζ *and diff. coëff.*, or as a linear function of the seventy-eight squares and products of these quantities. But the seventy-eight coëfficients by which this function is expressed will vary with the position of the element of volume with respect to the surrounding molecules.

In estimating the statical energy for any considerable space by the integral

$$\int \sigma dv,$$

it will be allowable to substitute for the seventy-eight coëfficients contained implicitly in σ their average values throughout the medium. That is, if we write s for a quadratic function of ξ, η, ζ, *and diff. coëff.* in which the seventy-eight coëfficients are the space-averages of those in σ, the statical energy of any considerable space may be estimated by the integral

$$\int s dv.$$

(This will appear most distinctly if we suppose the integration to be first effected for a thin slice of the medium bounded by two wave-planes.) The seventy-eight coëfficients of this function s are determined solely by the nature of the medium and the period of oscillation.

We may divide s into three parts, of which the first ($s_{,}$) contains the squares and products of ξ, η, ζ, the second ($s_{,,}$) contains the products of ξ, η, ζ with the differential coëfficients, and the third ($s_{,,,}$) contains the squares and products of the differential coëfficients. It is evident that the average statical energy of the whole medium per unit of volume is the space-average of s, and that it will consist of three parts, which are the space-averages of $s_{,}$, $s_{,,}$, and $s_{,,,}$, respectively. These parts we may call $S_{,}$, $S_{,,}$, and $S_{,,,}$. Only the first of these was considered in the preceding paper.

Now the considerations which justify us in neglecting, for an approximate estimate, the terms of s which contain the differential coëfficients of ξ, η, ζ with respect to the coördinates, will apply with especial force to the terms which contain the squares and products of these differential coëfficients. Therefore, to carry the approximation one step beyond that of the preceding paper, it will only be necessary to take account of $s_{,}$ and $s_{,,}$ and of $S_{,}$ and $S_{,,}$.

7. We may set

$$s_{,} = A\xi^2 + B\eta^2 + C\zeta^2 + E\eta\zeta + F\zeta\xi + G\xi\eta, \qquad (5)$$

where, for a given medium and light of a given period, A, B, C, E, F, G are constant.

Since the average values of

$$\sin^2 2\pi\frac{u}{l}, \quad \cos^2 2\pi\frac{u}{l}, \quad \sin 2\pi\frac{u}{l}\cos 2\pi\frac{u}{l}$$

are respectively $\frac{1}{2}$, $\frac{1}{2}$, and 0, and since at the time to be considered

$$\cos^2 2\pi\frac{t}{p}=1,$$

it will appear from inspection of equations (1) that

$$\begin{aligned}S_{/}=&\tfrac{1}{2}(A\alpha_1^2+B\beta_1^2+C\gamma_1^2+E\beta_1\gamma_1+F\gamma_1\alpha_1+G\alpha_1\beta_1)\\&+\tfrac{1}{2}(A\alpha_2^2+B\beta_2^2+C\gamma_2^2+E\beta_2\gamma_2+F\gamma_2\alpha_2+G\alpha_2\beta_2).\end{aligned}\qquad(6)$$

This is the first part of the statical energy of the whole medium per unit of volume.

8. The second part of the statical energy of the whole medium per unit of volume ($S_{//}$) is the space-average of $s_{//}$, which is a linear function of the twenty-seven products of ξ, η, ζ with their differential coëfficients with respect to the coördinates. Now since

$$\xi\frac{d\xi}{dx}=\tfrac{1}{2}\frac{d(\xi^2)}{dx}, \quad \eta\frac{d\eta}{dx}=\tfrac{1}{2}\frac{d(\eta^2)}{dx}, \quad \text{etc.},$$

the space-average of such products will be zero, and they will contribute nothing to the value of $S_{//}$. There will be nine of these products, in which the same component of displacement appears twice. The remaining eighteen products may be divided into pairs according to the letters which they contain, as

$$\eta\frac{d\zeta}{dx} \quad \text{and} \quad \zeta\frac{d\eta}{dx}.$$

A linear function of the eighteen products may also be regarded as a linear function of the sums and differences of the products in such pairs. But since

$$\eta\frac{d\zeta}{dx}+\zeta\frac{d\eta}{dx}=\frac{d(\eta\zeta)}{dx},$$

the terms of $s_{//}$ containing such sums will contribute nothing to the value of $S_{//}$. We have left a linear function of the nine differences

$$\eta\frac{d\zeta}{dx}-\zeta\frac{d\eta}{dx}, \quad \zeta\frac{d\xi}{dx}-\xi\frac{d\zeta}{dx}, \quad \xi\frac{d\eta}{dx}-\eta\frac{d\xi}{dx}, \quad \text{etc.},$$

(the unwritten expressions being obtained by substituting in the denominators dy and dz for dx,) which constitutes the part

of $s_{//}$ that we have to consider. $S_{//}$ is therefore a linear function of the space-averages of these nine quantities. But by (3)

$$\eta\frac{d\zeta}{dx}-\zeta\frac{d\eta}{dx}=\mathrm{L}\left(\eta\frac{d\zeta}{du}-\zeta\frac{d\eta}{du}\right),$$

and the space-average of this, at a moment of maximum displacement, is by (1)

$$\frac{2\pi\mathrm{L}}{l}(\beta_1\gamma_2-\gamma_1\beta_2).$$

By such reductions it appears that $lS_{//}$ is a linear function of the nine products of L, M, N with

$$\beta_1\gamma_2-\gamma_1\beta_2,\quad \gamma_1\alpha_2-\alpha_1\gamma_2,\quad \alpha_1\beta_2-\beta_1\alpha_2.$$

Now if we set

$$\Theta=\mathrm{L}(\beta_1\gamma_2-\gamma_1\beta_2)+\mathrm{M}(\gamma_1\alpha_2-\alpha_1\gamma_2)+\mathrm{N}(\alpha_1\beta_2-\beta_1\alpha_2), \quad (7)$$

we have by (4) and (2)

$$\mathrm{L}\Theta=\beta_1\gamma_2-\gamma_1\beta_2,\quad \mathrm{M}\Theta=\gamma_1\alpha_2-\alpha_1\gamma_2,\quad \mathrm{N}\Theta=\alpha_1\beta_2-\beta_1\alpha_2. \quad (8)$$

Therefore $lS_{//}$ is a linear function of the nine products of L, M, N with $\mathrm{L}\theta$, $\mathrm{M}\theta$, $\mathrm{N}\theta$. That is, $lS_{//}$ is the product of θ and a quadratic function of L, M and N. We may therefore write

$$S_{//}=\frac{\Phi}{l}\Theta=\frac{\Phi}{l}[\mathrm{L}(\beta_1\gamma_2-\gamma_1\beta_2)+\mathrm{M}(\gamma_1\alpha_2-\alpha_1\gamma_2)+\mathrm{N}(\alpha_1\beta_2-\beta_1\alpha_2)], \quad (9)$$

where Φ is a quadratic function of L, M and N, dependent, however, on the nature of the medium and the period of oscillation.

9. It will be useful to consider more closely the geometrical significance of the quantity θ. For this purpose it will be convenient to have a definite understanding with respect to the relative position of the coördinate axes.

We shall suppose that the axes of X, Y, and Z are related in the same way as lines drawn to the right, forward and upward, so that a rotation from X to Y appears clock-wise to one looking in the direction of Z.

Now if from any same point, as the origin of coördinates, we lay off lines representing in direction and magnitude the displacements in all the different wave-planes, we obtain an ellipse, which we may call the *displacement-ellipse.** Of this, one radius vector (ρ_1) will have the components α_1, β_1, γ_1, and

* This ellipse, which represents the simultaneous displacements in different parts of the field, will also represent the successive displacements at any same point in the corresponding system of progressive waves.

another (ρ_2) the components α_2, β_2, γ_2. These will belong to conjugate diameters, each being parallel to the tangent at the extremity of the other. The area of the ellipse will therefore be equal to the parallelogram of which ρ_1, and ρ_2 are two sides, multiplied by π. Now it is evident that $\beta_1\gamma_2-\gamma_1\beta_2$, $\gamma_1\alpha_2-\alpha_1\gamma_2$, $\alpha_2\beta_1-\beta_1\alpha_2$ are numerically equal to the projections of this parallelogram on the planes of the coördinate axes, and are each positive or negative according as a revolution from ρ_1 to ρ_2 appears clock-wise or counter-clock-wise to one looking in the direction of the proper coördinate axis. Hence, θ will be numerically equal to the parallelogram, that is, to the area of the displacement-ellipse divided by π, and will be positive or negative according as a revolution from ρ_1 to ρ_2 appears clock-wise or counter-clock-wise to one looking in the direction of the wave-normal. Since ρ_1 and ρ_2 are determined by displacements in planes one-quarter of a wave-length distant from each other, and the plane to which the latter relates lies on the side toward which the wave-normal is drawn, it follows that θ is positive or negative according as the combination of displacements has the character of a right-handed or a left-handed screw.

10. The kinetic energy of the medium, which is to be estimated for an instant of no displacement, may be shown as in § 7 of the former paper (page 266 of this volume) to consist of two parts, of which one relates to the regular flux ($\dot\xi$, $\dot\eta$, $\dot\zeta$), and the other to the irregular flux ($\dot\xi'$, $\dot\eta'$, $\dot\zeta'$). The first, in the notation of that paper, is represented by

$$\tfrac{1}{2}f'(\dot\xi\ \mathrm{Pot}\ \dot\xi+\dot\eta\ \mathrm{Pot}\ \dot\eta+\dot\zeta\ \mathrm{Pot}\ \dot\zeta)\,dv,$$

which reduces to

$$\frac{l^2}{2\pi}f(\dot\xi^2+\dot\eta^2+\dot\zeta^2)\,dv.$$

By substitution of the values given by equations (1), we obtain for the kinetic energy due to the regular flux in a unit of volume

$$\mathrm{T}=\frac{\pi l^2}{\rho^2}(\alpha_1^2+\beta_1^2+\gamma_1^2+\alpha_2^2+\beta_2^2+\gamma_2^2). \qquad (10)$$

11. The kinetic energy of the irregular part of the flux is represented by the volume-integral

$$f\tfrac{1}{2}(\dot\xi'\ \mathrm{Pot}\ \dot\xi'+\dot\eta'\ \mathrm{Pot}\ \dot\eta'+\dot\zeta'\ \mathrm{Pot}\ \dot\zeta')\,dv.$$

Now, since $\dot\xi'$, $\dot\eta'$, $\dot\zeta'$ are everywhere linear functions of $\dot\xi$, $\dot\eta$, $\dot\zeta$ *and diff. coëff.* (see § 4), and since the integrations implied in the notation *Pot* may be confined to a sphere of which the

radius is small in comparison with a wave-length,* and since within such a sphere $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$ *and diff. coëff.* are sufficiently determined (in a linear form), by the values of the same twelve quantities at the center of the sphere, it follows that Pot $\dot{\xi}'$, Pot $\dot{\eta}'$, Pot $\dot{\zeta}'$ must be linear functions of the values of $\dot{\xi}$, $\dot{\eta}'$, $\dot{\zeta}$ *and diff. coëff.* at the point for which the potential is sought. Hence,

$$\tfrac{1}{2}(\dot{\xi}' \text{ Pot } \dot{\xi}' + \dot{\eta}' \text{ Pot } \dot{\eta}' + \dot{\zeta}' \text{ Pot } \dot{\zeta}')$$

will be a quadratic function of $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$ *and diff. coëff.* But the seventy-eight coëfficients by which this function is expressed will vary with the position of the point considered with respect to the surrounding molecules.

Yet, as in the case of the statical energy, we may substitute the average values of these coëfficients for the coëfficients themselves in the integral by which we obtain the energy of any considerable space. The kinetic energy due to the irregular part of the flux is thus reduced to a quadratic function of $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$ *and diff. coëff.* which has constant coëfficients for a given medium and light of a given period.

The function may be divided into three parts, of which the first contains the squares and products of $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$, the second the products of $\dot{\xi}$, $\dot{\eta}$, $\dot{\zeta}$ with their differential coëfficients, and the third, which may be neglected, the squares and products of the differential coëfficients.

We may proceed with the reduction precisely as in the case of the statical energy, except that the differentiations with respect to the time will introduce the constant factor $\frac{4\pi^2}{p^2}$. This will give for the first part of the kinetic energy of the irregular flux per unit of volume

$$\mathrm{T}'_{,} = \frac{2\pi^2}{p^2}(\mathrm{A}'\alpha_1^2 + \mathrm{B}'\beta_1^2 + \mathrm{C}'\gamma_1^2 + \mathrm{E}'\beta_1\gamma_1 + \mathrm{F}'\gamma_1\alpha_1 + \mathrm{G}'\alpha_1\beta_1)$$
$$+ \frac{2\pi^2}{p^2}(\mathrm{A}'\alpha_2^2 + \mathrm{B}'\beta_2^2 + \mathrm{C}'\gamma_2^2 + \mathrm{E}'\beta_2\gamma_2 + \mathrm{F}'\gamma_2\alpha_2 + \mathrm{G}'\alpha_2\beta_2), \quad (11)$$

and for the second part of the same

$$\mathrm{T}'_{,,} = \frac{4\pi^2\Phi'}{p^2 l}\Theta$$
$$= \frac{4\pi^2\Phi'}{p^2 l}[\mathrm{L}(\beta_1\gamma_2 - \gamma_1\beta_2) + \mathrm{M}(\gamma_1\alpha_2 - \alpha_1\gamma_2) + \mathrm{N}(\alpha_1\beta_2 - \beta_1\alpha_2)], \quad (12)$$

* See § 9 of the former paper, on page 268 of this volume.

where A′, B′, C′, E′, F′, G′ are constant, and Φ' a quadratic function of L, M, and N, for a given medium and light of a given period.

12. Equating the statical and kinetic energies, we have

$$S_{,}+S_{,,}=T+T'_{,}+T'_{,,},$$

that is, by equations (6), (9), (10), (11), and (12),

$$\begin{aligned}
&\tfrac{1}{2}(A\alpha_1^2+B\beta_1^2+C\gamma_1^2+E\beta_1\gamma_1+F\gamma_1\alpha_1+G\alpha_1\beta_1)\\
&+\tfrac{1}{2}(A\alpha_2^2+B\beta_2^2+C\gamma_2^2+E\beta_2\gamma_2+F\gamma_2\alpha_2+G\alpha_2\beta_2)\\
&+\frac{\Phi}{l}[L(\beta_1\gamma_2-\gamma_1\beta_2)+M(\gamma_1\alpha_2-\alpha_1\gamma_2)+N(\alpha_1\beta_2-\beta_1\alpha_2)\\
&=\frac{\pi l^2}{p^2}(\alpha_1^2+\beta_1^2+\gamma_1^2+\alpha_2^2+\beta_2^2+\gamma_2^2)\\
&+\frac{2\pi^2}{p^2}(A'\alpha_1^2+B'\beta_1^2+C'\gamma_1^2+E'\beta_1\gamma_1+F'\gamma_1\alpha_1+G'\alpha_1\beta_1)\\
&+\frac{2\pi^2}{p^2}(A'\alpha_2^2+B'\beta_2^2+C'\gamma_2^2+E'\beta_2\gamma_2+F'\gamma_2\alpha_2+G'\alpha_2\beta_2)\\
&+\frac{4\pi^2\Phi'}{p^2 l}[L(\beta_1\gamma_2-\gamma_1\beta_2)+M(\gamma_1\alpha_2-\alpha_1\gamma_2)+N(\alpha_1\beta_2-\beta_1\alpha_2). \quad (13)
\end{aligned}$$

If we set

$$a=\frac{A}{2\pi}-\frac{2\pi A'}{p^2}, \quad b=\frac{B}{2\pi}-\frac{2\pi B'}{p^2}, \quad \text{etc.}, \qquad (14)$$

and

$$\varphi=\frac{\Phi}{2\pi p}-\frac{2\pi\Phi'}{p^3}, \qquad (15)$$

the equation reduces to

$$\begin{aligned}
&a\alpha_1^2+b\beta_1^2+c\gamma_1^2+e\beta_1\gamma_1+f\gamma_1\alpha_1+g\alpha_1\beta_1\\
&+a\alpha_2^2+b\beta_2^2+c\gamma_2^2+e\beta_2\gamma_2+f\gamma_2\alpha_2+g\alpha_2\beta_2\\
&+\frac{2p\varphi}{l}[L(\beta_1\gamma_2-\gamma_1\beta_2)+M(\gamma_1\alpha_2-\alpha_1\gamma_2)+N(\alpha_1\beta_2-\beta_1\alpha_2)]\\
&=\frac{l^2}{p^2}(\alpha_1^2+\beta_1^2+\gamma_1^2+\alpha_2^2+\beta_2^2+\gamma_2^2), \qquad (16)
\end{aligned}$$

where a, b, c, e, f, g are constant, and φ a quadratic function of L, M, N, for a given medium and light of a given period.

13. Now this equation, which expresses a relation between the constants of the equations of wave-motion (1), will apply, with those equations, not only to such vibrations as actually take place, but also to such as we may imagine to take place under the influence of constraints determining the type of vibration. The free or unconstrained vibrations, with which alone we are concerned, are characterized by this, that infinitesimal variations (by constraint) of the type of vibration,

that is, of the ratios of the quantities $\alpha_1, \beta_1, \gamma_1, \alpha_2, \beta_2, \gamma_2$, will not affect the period by any quantity of the same order of magnitude.* These variations must however be consistent with equations (4), which require that

$$\mathrm{L}d\alpha_1+\mathrm{M}d\beta_1+\mathrm{N}d\gamma_1=0, \quad \mathrm{L}d\alpha_2+\mathrm{M}d\beta_2+\mathrm{N}d\gamma_2=0. \tag{17}$$

Hence, to obtain the conditions which characterize free vibration, we may differentiate equation (16) with respect to $\alpha_1, \beta_1, \gamma_1, \alpha_2, \beta_2, \gamma_2$ regarding all other letters as constant, and give to $d\alpha_1, d\beta_1, d\gamma_1, d\alpha_2, d\beta_2, d\gamma_2$, such values as are consistent with equations (17). Now $d\alpha_1, d\beta_1, d\gamma_1$, are independent of $d\alpha_2, d\beta_2, d\gamma_2$, and for either three variations, values proportional either to $\alpha_1, \beta_1, \gamma_1$, or to $\alpha_2, \beta_2, \gamma_2$, are possible. If, then, we differentiate equation (16) with respect to $\alpha_1, \beta_1, \gamma_1$, and substitute first $\alpha_1, \beta_1, \gamma_1$, and then $\alpha_2, \beta_2, \gamma_2$, for $d\alpha_1, d\beta_1, d\gamma_1$, and also differentiate with respect to $\alpha_2, \beta_2, \gamma_2$, with similar substitutions, we shall obtain all the independent equations which this principle will yield.

If we differentiate with respect to $\alpha_1, \beta_1, \gamma_1$, and write $\alpha_1, \beta_1, \gamma_1$ for $d\alpha_1, d\beta_1, d\gamma_1$, we obtain

$$\begin{aligned}&a\alpha_1^2+b\beta_1^2+c\gamma_1^2+e\beta_1\gamma_1+f\gamma_1\alpha_1+g\alpha_1\beta_1\\&+\frac{p\varphi}{l}[\mathrm{L}(\beta_1\gamma_2-\gamma_1\beta_2)+\mathrm{M}(\gamma_1\alpha_2-\alpha_1\gamma_2)+\mathrm{N}(\alpha_1\beta_2-\beta_1\alpha_2)]\\&=\frac{l^2}{p^2}(\alpha_1^2+\beta_1^2+\gamma_1^2).\end{aligned} \tag{18}$$

If we differentiate with respect to $\alpha_1, \beta_1, \gamma_1$, and write $\alpha_2, \beta_2, \gamma_2$ for $d\alpha_1, d\beta_1, d\gamma_1$, we obtain

$$\begin{aligned}&2a\alpha_1\alpha_2+2b\beta_1\beta_2+2c\gamma_1\gamma_2+e(\beta_1\gamma_2+\gamma_1\beta_2)+f(\gamma_1\alpha_2+\alpha_1\gamma_2)\\&\qquad+g(\alpha_1\beta_2+\beta_1\alpha_2)=\frac{2l^2}{p^2}(\alpha_1\alpha_2+\beta_1\beta_2+\gamma_1\gamma_2).\end{aligned} \tag{19}$$

If we differentiate with respect to $\alpha_2, \beta_2, \gamma_2$, and write $\alpha_2, \beta_2, \gamma_2$ for $d\alpha_2, d\beta_2, d\gamma_2$, we obtain

$$\begin{aligned}&a\alpha_2^2+b\beta_2^2+c\gamma_2^2+e\beta_2\gamma_2+f\gamma_2\alpha_2+g\alpha_2\beta_2\\&+\frac{p\varphi}{l}[\mathrm{L}(\beta_1\gamma_2-\gamma_1\beta_2)+\mathrm{M}(\gamma_1\alpha_2-\alpha_1\gamma_2)+\mathrm{N}(\alpha_1\beta_2-\beta_1\alpha_2)]\\&=\frac{l^2}{p^2}(\alpha_2^2+\beta_2^2+\gamma_2^2).\end{aligned} \tag{20}$$

The equation derived by differentiating with respect to $\alpha_2, \beta_2, \gamma_2$, and writing $\alpha_1, \beta_1, \gamma_1$ for $d\alpha_2, d\beta_2, d\gamma_2$, is identical with (19). We should also observe that equations (18) and (20) by addi-

* Compare § 11 of the former paper, page 270 of this volume.

tion give equation (16), which therefore will not need to be considered in addition to the last three equations.

14. The geometrical signification of our equations may now be simplified by a suitable choice of the position of the origin of coördinates, which is as yet wholly arbitrary.

We shall hereafter suppose that the origin is placed in a plane of maximum or minimum displacement,* if such there are. In the case of circular polarization, in which the displacements are everywhere equal, its position is immaterial. The lines ρ_1 and ρ_2, of which α_1, β_1, γ_1 and α_2, β_2, γ_2 are respectively the components, will now be the semi-axes of the displacement-ellipse, and therefore at right angles. (See § 9.) The case of circular polarization will not constitute any exception. Hence,

$$\alpha_1\alpha_2+\beta_1\beta_2+\gamma_1\gamma_2=0. \tag{21}$$

and by § 9,

$$\Theta=\mathrm{L}(\beta_1\gamma_2-\gamma_1\beta_2)+\mathrm{M}(\gamma_1\alpha_2-\alpha_1\gamma_2)+\mathrm{N}(\alpha_1\beta_2-\beta_1\alpha_2)=\pm\rho_1\rho_2, \tag{22}$$

where we are to read + or − in the last member according as the system of displacements has the character of a right-handed or a left-handed screw.

15. Equation (19) is now reduced to the form

$$\begin{aligned}2a\alpha_1\alpha_2+2b\beta_1\beta_2+2c\gamma_1\gamma_2+e(\beta_1\gamma_2+\gamma_1\beta_2)\\ +f(\gamma_1\alpha_2+\alpha_1\gamma_2)+g(\alpha_1\beta_2+\beta_1\alpha_2)=0,\end{aligned} \tag{23}$$

which has a very simple geometrical signification. If we consider the ellipsoid

$$ax^2+by^2+cz^2+eyz+fzx+gxy, \tag{24}$$

and especially its central section by a plane parallel to the planes of the wave-system which we are considering, it will easily appear that the equation

$$\begin{aligned}2ax_1x_2+2by_1y_2+2cz_1z_2+e(y_1z_2+z_1y_2)\\ +f(z_1x_2+x_1z_2)+g(x_1y_2+y_1x_2)=0\end{aligned}$$

will hold of any two points x_1, y_1, z_1 and x_2, y_2, z_2 which belong to conjugate diameters of this central section. Therefore equation (23) expresses that the displacements α_1, β_1, γ_1 and α_2, β_2, γ_2 are parallel to conjugate diameters of the central section of the ellipsoid (24) by a wave-plane. But since the displacements α_1, β_1, γ_1 and α_2, β_2, γ_2 are also at right angles to each other, it follows that they are parallel to the axes of the central section of the ellipsoid (24) by a wave-plane. That is:—

* The reader will perceive that an earlier limitation of the position of the origin by a supposition of this nature, involving a limitation of the values of α_1, β_1, γ_1, α_2, β_2, γ_2, would have been embarrassing in the operations of the last paragraph.

The axes of the displacement-ellipse coincide in direction with those of a central section of the ellipsoid (24) by a wave-plane.

16. If we write U_1, U_2 for the reciprocals of the semi-axes of the central section of the ellipsoid (24) by a wave-plane, U_1 being the reciprocal of the one to which the displacement α_1, β_1, γ_1 is parallel, we have

$$a\alpha_1^2+b\beta_1^2+c\gamma_1^2+e\beta_1\gamma_1+f\gamma_1\alpha_1+g\alpha_1\beta_1=U_1^2(\alpha_1^2+\beta_1^2+\gamma_1^2), \quad (25)$$

as is at once evident, if we substitute the coördinates of an extremity of the axis, for the proportional quantities α_1, β_1, γ_1. So also

$$a\alpha_2^2+b\beta_2^2+c\gamma_2^2+e\beta_2\gamma_2+f\gamma_2\alpha_2+g\alpha_2\beta_2=U_2^2(\alpha_2^2+\beta_2^2+\gamma_2^2). \quad (26)$$

If we write V for the velocity of propagation of the system of progressive waves corresponding to the system of stationary waves which we have been considering, we shall have

$$V=\frac{l}{p}. \quad (27)$$

By equations (22), (25), and (26), equations (18) and (20) are reduced to the form

$$U_1^2\rho_1^2\pm\frac{\varphi}{V}\rho_1\rho_2=V^2\rho_1^2, \quad U_2^2\rho_2^2\pm\frac{\varphi}{V}\rho_1\rho_2=V^2\rho_2^2, \quad (28)$$

where we are to read + or − according as the disturbance has the character of a right-handed or a left-handed screw. In a progressive system of waves, when the combination of displacements has the character of a right-handed screw, the rotations will be such as appear clock-wise to the observer, who looks in the direction opposite to that of the propagation of light. We shall call such a ray *right-handed*.

We may here observe that in case $\varphi=0$ the solution of these equations is very simple. We have necessarily either $\rho_2=0$ and $V^2=U_1^2$, or $\rho_1=0$ and $V^2=U_2^2$. In this case, the light is linearly polarized, and the directions of oscillation and the velocities of propagation are given by Fresnel's law. Experiment has shown that this is the usual case. We wish, however, to investigate the case in which φ does not vanish. Since the term containing φ arises from the consideration of those quantities which it was allowable to neglect in the first approximation, we may assume that φ is always very small in comparison with V^3, U_1^3, or U_2^3.

17. Equations (28) may be written

$$V^2-U_1^2=\pm\frac{\varphi}{V}\frac{\rho_2}{\rho_1}, \quad V^2-U_2^2=\pm\frac{\varphi}{V}\frac{\rho_1}{\rho_2}. \quad (29)$$

By multiplication we obtain

$$V^2(V^2-U_1^2)(V^2-U_2^2)=\varphi^2. \tag{30}$$

Since φ is a very small quantity, it is evident from inspection of this equation that it will admit three values of V^2, of which one will be a very little greater than the greater of the two quantities U_1^2 and U_2^2, another will be a very little less than the less of the same two quantities, and the third will be a very small quantity. It is evident that the values of V^2 with which we have to do are those which differ but little from U_1^2 and U_2^2.*

For the numerical computation of V, when U_1, U_2, and φ are known numerically, we may divide the equation by V^2, and then solve it as if the second member were known. This will give

$$V^2=\frac{U_1^2+U_2^2}{2}\pm\sqrt{\frac{\varphi^2}{V^2}+\frac{(U_1^2-U_2^2)^2}{4}}. \tag{31}$$

By substituting U_1U_2 for V^2 in the second member, we may obtain a close approximation to the two values of V^2. Each of the values obtained may be improved by substitution of that value for V^2 in the second member of the equation.

For either value of V^2, we may easily find the ratio of ρ_1 to ρ_2, that is, the ratio of the axes of the displacement-ellipse, from one of equations (29), or from the equation

$$\frac{\rho_2^2}{\rho_1^2}=\frac{V^2-U_1^2}{V^2-U_2^2} \tag{32}$$

obtained by combining the two.

In equations (29), we are to read + or − in the second members, according as the ray is right-handed or left-handed. (See § 16.) It follows that if the value of φ is positive, the greater velocity will belong to a right-handed ray, and the smaller to a left-handed, but if the value of φ is negative, the opposite is the case. Except when $\varphi=0$, and the polarization is linear, there will be one right-handed and one left-handed ray for any given wave-normal and period.

18. When $U_1=U_2$, equations (29) give

$$\rho_1=\rho_2, \quad V^2=U^2\pm\frac{\varphi}{V},$$

* We should not attribute any physical significance to the third value of V^2. For this value would imply a wave-length very small in comparison with the length of ordinary waves of light, and with respect to which our fundamental assumption that the wave-length is very great in comparison with the distances of contiguous molecules would be entirely false. Our analysis, therefore, furnishes no reason for supposing that any such velocities are possible for the propagation of electrical disturbances.

where U represents the common value of U_1 and U_2. The polarization is therefore circular. The converse is also evident from equations (29), viz: that a ray can be circularly polarized only when the direction of its wave-normal is such that $U_1 = U_2$. Such a direction, which is determined by a circular section of the ellipsoid (24) precisely as an optic axis of a crystal which conforms to Fresnel's law of double refraction, may be called an optic axis, although its physical properties are not the same as in the more ordinary case.* If we write V_R and V_L, respectively, for the wave-velocities of the right-handed and left-handed rays, we have

$$V_R^2 = U^2 + \frac{\varphi}{V_R}, \quad V_L^2 = U^2 - \frac{\varphi}{V_L}; \tag{33}$$

whence

$$V_R^2 - V_L^2 = \varphi\left(\frac{1}{V_R} + \frac{1}{V_L}\right) = \varphi\frac{V_R + V_L}{V_R V_L},$$

and

$$V_R - V_L = \frac{\varphi}{V_R V_L}. \tag{34}$$

The phenomenon best observed with respect to an optic axis is the rotation of the plane of linearly polarized light. If we denote by θ the amount of this rotation per unit of the distance traversed by the wave-plane, regarding it as positive when it appears clock-wise to the observer, who looks in the direction opposite to that of the propagation of the light,† we have

$$\theta = \frac{\pi}{p}\left(\frac{1}{V_L} - \frac{1}{V_R}\right), \tag{35}$$

By the preceding equation, this reduces to

$$\theta = \frac{\pi\varphi}{pV_R^2 V_L^2}. \tag{36}$$

*Our experimental knowledge of circularly or elliptically polarizing media is confined to such as are optically either isotropic or uniaxial. The general theory of such media, embracing the case of two optic axes, has however been discussed by Professor von Lang. (*Theorie der Circularpolarization*, Sitz.-Ber. Wiener Akad., vol. lxxv, p. 719.) The general results of the present paper, although derived from physical hypotheses of an entirely different nature, are quite similar to those of the memoir cited. They would become identical, the writer believes, by the substitution of a constant for $\frac{p\phi}{l}$ or $\frac{\phi}{V}$ in the equations of this paper. [See especially equations (18), (20), (28).]

That a complete discussion of the subject on any theory must include the case of biaxial media having the property of circular or elliptical polarization, is evident from the consideration that it must at least be possible to produce examples of such media artificially. An isotropic or uniaxial crystal may be made biaxial by pressure. If it has the property of circular and elliptic polarization, that property cannot be wholly destroyed by the application of small pressures.

†When the rotation of the plane of polarization appears clock-wise to the observer, it has the character of a *left-handed* screw. But the circularly polarized ray to which V_R relates, the rotation of which also appears clock-wise to the observer, has the character of a *right-handed* screw.

Without any appreciable error, we may substitute U^4 for $V_R^2 V_L^2$, which will give*

$$\theta=\frac{\pi\Phi}{p\overline{U}^4}. \tag{37}$$

19. Since these equations involve unknown functions of the period, they will not serve for an exact determination of the relation between θ and the period. For a rough approximation, however, we may assume that the manner in which the general displacement in any small part of the medium distributes itself among the molecules and intermolecular spaces is independent of the period, being determined entirely by the values of ξ, η, ζ, and their differential coëfficients with respect to the coördinates.† For a fixed direction of the wave-normal, Φ and Φ' will then be constant. Now equations (15) and (36) give

$$\theta=\frac{\Phi}{2p^2V_R^2V_L^2}-\frac{2\pi^2\Phi'}{p^4V_R^2V_L^2} \tag{38}$$

To express this result in terms of the quantities directly observed, we may use the equations

$$p=\frac{\lambda}{k},\quad V_R=\frac{k}{n_R},\quad V_L=\frac{k}{n_L},\quad U=\frac{k}{n},$$

where k denotes the velocity of light *in vacuo*, λ the wave-length *in vacuo* of the light employed, n_R, n_L the absolute indices of refraction of the two rays, and n the index for the optic axis as derived from the ellipsoid (24) by Fresnel's law. We thus obtain

$$\theta=\frac{\Phi n_R^2 n_L^2}{2k^2\lambda^2}-\frac{2\pi^2\Phi' n_R^2 n_L^2}{\lambda^4} \tag{39}$$

In the case of uniaxial crystals, the direction of the optic axis is fixed. We may therefore write

$$\theta=n_R^2 n_L^2\left(\frac{K}{\lambda^2}+\frac{K'}{\lambda^4}\right), \tag{40}$$

regarding K and K′ as constants. If we had used equation (37), we should have had the factor n^4 instead of $n_R^2 n_L^2$.

* The degree of accuracy of this substitution may be shown as follows. By (33)

$$V_R(V_R^2-U^2)=V_L(U^2-V_L^2),$$

whence

$$V_R^2+V_L^2=(V_R+V_L)U^2,$$
$$V_R^2-V_RV_L+V_L^2=U^2,$$
$$V_RV_L=U^2-(V_R-V_L)^2.$$

† Compare § 12 of the former paper, on page 270 of this volume.

Since this factor varies but slowly with λ, it may be neglected, if its omission is compensated in the values of K and K′. The formula being only approximative, such a simplification will not necessarily render it less accurate.

20. But without any such assumption as that contained in the last paragraph, we may easily obtain formulæ for the experimental determination of Φ and Φ' for the optic axis of an uniaxial crystal. Considerations analogous to those of § 13 of the former paper (page 271 of this volume), show that in differentiating equation (39) we may regard Φ and Φ' as constant, although they may actually vary with λ. This equation may be written

$$\frac{\theta\lambda^2}{n^4}=\frac{\Phi}{2k^2}-\frac{2\pi^2\Phi'}{\lambda^2} \tag{41}$$

Therefore,

$$\frac{d\left(\frac{\theta\lambda^2}{n^4}\right)}{d\left(\frac{1}{\lambda^2}\right)}=-2\pi^2\Phi'. \tag{42}$$

When Φ' has been determined by this equation, Φ may be found from the preceding.

21. If we wish to represent φ geometrically, like U_1 and U_2, we may construct the surfaces

$$Ax^2+By^2+Cz^2+Eyz+Fzx+Gxy=\pm 1, \tag{43}$$

the coëfficients A, B, etc., being the same by which φ is expressed in terms of L^2, M^2, etc. The numerical value of φ, for any direction of the wave-normal, will thus be represented by the square of the reciprocal of the radius vector of the surface drawn in the same direction. The positive or negative character of φ must be separately indicated. There are here two cases to be distinguished. If the sign of φ is the same in all directions, the surface will be an ellipse, and we have only to know whether all the values of φ are to be taken positively or all negatively. But if φ is positive for some directions and negative for others, the surface will consist of two conjugate hyperboloids, to one of which the positive, and to the other the negative values belong.

22. The manner in which the ellipsoid (24) may be partially determined by the relations of symmetry which the medium may possess, has been sufficiently discussed in the former paper.

With respect to the quantity φ, and the surfaces which determine it, the following principle is of fundamental importance. If one body is identical in its internal structure with the image by reflection of another, the values of φ in corres-

ponding lines in the two bodies will be numerically equal but have opposite signs.*

It follows that if a body is identical in internal structure with its own image by reflection, the value of φ (if not zero for all directions) must be positive for some directions and negative for others. Moreover, the above described surface by which φ is represented must consist of two conjugate hyperboloids, of which one is identical in form with the image by reflection of the other. This requires that the hyperboloids shall be right cylinders with conjugate rectangular hyperbolas for bases. A crystal characterized by such properties will belong to the tetragonal system. Since $\varphi=0$ for the optic axis, it would be difficult to distinguish a case of this kind from an ordinary uniaxial crystal, unless the ellipsoid (24) should approach very closely to a sphere.†

It is only in the very limited case described in the last paragraph that a medium which is identical in its internal structure with its image by reflection can have the property of circular or elliptic polarization. To media which are unlike their images by reflection, and have the property of circular polarization, we may apply the following general principles.

If the medium has any axis of symmetry, the ellipsoid or hyperboloids which represent the values of φ will have an axis in the same direction. If the medium after a revolution of less than 180° about any axis is equivalent to the medium in its first position, the ellipsoid or hyperboloids will have an axis of revolution in that direction.

23. The laws of the propagation of light in plane waves, which have thus been derived from the single hypothesis that the disturbance by which light is transmitted consists of solenoidal electrical fluxes, and which apply to light of different colors and to the most general case of perfectly transparent and sensibly homogeneous media not subject to magnetic action,‡ are essentially those which are generally received as

* The necessity of the opposite signs will perhaps appear most readily from the consideration that the direction of rotation of the plane of polarization must be opposite in the two bodies.

† There is no difficulty in conceiving of the constitution of a body which would have the properties described above. Thus, we may imagine a body with molecules of a spiral form, of which one-half are right-handed and one-half left-handed, and we may suppose that the motion of electricity is opposed by a less resistance within them than without. If the axes of the right-handed molecules are parallel to the axis of X, and those of the left-handed molecules to the axis of Y, their effects would counterbalance one another when the wave-normal is parallel to the axis of Z. But when the wave-normal (of a beam of linearly polarized light), is parallel to the axis of X, the left-handed molecules would produce a left-handed (negative) rotation of the plane of polarization, the right-handed molecules having no effect; and when the wave-normal is parallel to the axis of Y, the reverse would be the case.

‡ The rotation of the plane of polarization which is produced by magnetic action has been discussed by Maxwell (*Treatise on Electricity and Magnetism*, vol. ii, Chap. XXI), and by Rowland (Amer. Journ. Math., vol. iii, p. 107).

embodying the results of experiment. In no particular, so far as the writer is aware, do they conflict with the results of experiment, or require the aid of auxiliary and forced hypotheses to bring them into harmony therewith.

In this respect, the electromagnetic theory of light stands in marked contrast with that theory in which the properties of an elastic solid are attributed to the ether,—a contrast which was very distinct in Maxwell's derivation of Fresnel's laws from electrical principles, but becomes more striking as we follow the subject farther into its details, and take account of the want of absolute homogeneity in the medium, so as to embrace the phenomena of the dispersion of colors and circular and elliptical polarization.

ART. LV.—*New Phyllopod Crustaceans from the Devonian of Western New York;* by JOHN M. CLARKE, Smith College. With a Plate (unnumbered.)

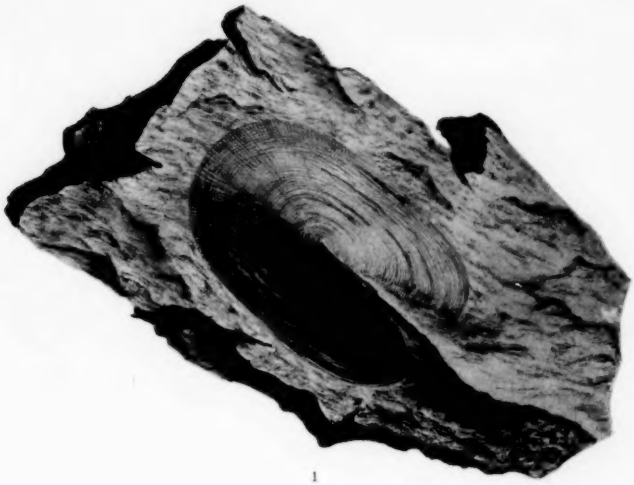
Estheria pulex, n. sp. Plate, fig. 4.

IN examining some fragments of soft, olive-colored shale from near the base of the Hamilton proper, in Miles' Gully, Hopewell, Ontario Co., I have detected the above representative of this extremely interesting genus. The little carapaces are never more than $\frac{2}{3}^{mm}$ in width and $\frac{1}{2}^{mm}$ in length, and may be described as having the ventral margin nearly semi-circular, the beak central or very slightly anterior, hinge line sloping laterally. The surface is marked by six, or in the largest seven, concentric ridges which are very broad with narrow intervening furrows. There appears to be no more elaborate sculpturing of the carapaces than Jones has figured for his species *E. membranacea*, which is the simplest of any as yet noticed.

It is interesting to notice that this *Estheria*, the first ever found below the Trias in America and nowhere at so low an horizon as this, resembles in its sub-central beak, its outline and surface markings, this species just referred to, *E. membranacea* Jones, from the Old Red of Caithness, while all others figured by that author (Mon. Esth. Paleontogr. Soc., vol. xviii) are from higher horizons, have the beak anterior, and the outline of the carapace more nearly sub-trigonal.

All recent *Estheriæ* are found in fresh, or, in possible cases, brackish waters. In some general remarks in this connection Jones has said that "seeing that *Estheriæ* appear in pools and ditches of rain-water, it is not unlikely that pools of fresh water temporarily formed on a flat sea-shore may have been inhabited

Face p. 476



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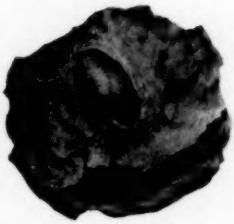

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J. M. C. & R. L. A. DEL.

ARTOTYPE, E. BIERSTADT, N. Y.

1. 2. 3. Spathiocaris Emersonii Clarke
4. Estheria pulex ,,
5. Liagocaris Lutheri ,,

by *Estheriæ*, destined to be quickly buried in the first wind-drift of sand on the return of high tide." We can not prove from this species, *E. pulex*, the presence of fresh-water deposits in the Hamilton, but the evidence is fairly in favor of brackish water pools, guarded from the action of the waves of the open ocean and depositing a smooth muddy bottom. Associated with these clannish little fellows are undescribed species of *Beyrichia*, *Leperditia*, *Entomis*, with a species of *Discina* not identified, and this association, omitting the *Discina*, is not an impossible one for such a brackish water pond. Even the *Discina* itself is not a serious objection to it, as I have found but a single example, which may have been washed in either as a dead shell, or as a live one, and if the latter, may have soon yielded to its unfavorable environment and died, or have adapted itself to the change and have lived on in an abnormal condition, as this specimen seems to prove.

Spathiocaris, n. g. (σπάθη=a spathe.) Plate, figs. 1, 2, 3.

Carapace in one piece (?), oblong-elliptical; dimensions when normal, length : width :: 3 : 2. Anterior and posterior marginal curvature of the same value except near the sides of the fissure on the posterior extremity where for a short distance the ouline becomes rectilinear. The central portion of the carapace elevated, the apical point being nearly a focus of the ellipse, and from this point starts a cleft extending backward and regularly widening to the posterior margin. No evidence of dorsal suture except in the fact that when flattened laterally, as many specimens are, the line of folding is usually straight from apex to anterior margin, but there is no external mark of a commissure in the ornamentation lines on the surface. It has for several years been a matter of some doubt to myself, whether this fossil should be looked upon as crustacean, which I now believe it to be, or some new form of Discinoid brachiopod which it very closely resembles, but my reasons for believing it the former lie in the fact that while I have in my possession thirty specimens, all that have as yet been found and all from a layer only a few inches in thickness, they show a great variation in size from a length of four to sixty millimeters, a fragment of an unusually large individual showing a probable length of even eighty or ninety millimeters, which is not a fact to expect from the Discinoid brachiopods; and, moreover, every specimen that is well preserved shows the cleft in the carapace, so that among them all there is no evidence of the ventral or non-fissured valve which we should expect to find if the animal had been brachiopodous. The genus *Discinocaris* described by Woodward from the Moffat shales of Dumfriesshire (Quart. Jour. Geol. Soc., vol. xxii), agrees with *Spathiocaris* in the

anchylosis of the lateral valves and its wide, wedge-shaped cleft, but differs in the presence of the "rostrum" or plate acting as another valve to cover the cleft, and also in its more nearly circular outline.

The ornamentation of the surface of the carapace in this species, *S. Emersonii*, n. sp., consists of low concentric ridges which never lose their continuity, but contiguous ridges are more closely appressed on the sides than at the extremities of the valves. At the edges of the cleft these lines show a tendency to a retral bending.

Radiating lines from the apex of the cleft to the margin cover the anterior portion of the carapace and give to this part a strong decussate sculpturing which is rarely noticeable along the margins of the cleft. I am not satisfied that I have seen the abdominal arthromeres of this species, though it will be only a matter of time for their detection.

The geological horizon in which this occurs, the Portage, is usually regarded as barren of fossils in New York, but is showing under careful scrutiny many facts of paleontological interest and bids fairly especially in the direction of the phyllopod crustacea.

From Naples, Ontario Co., N. Y.

Fig. 1. *Spathiocaris Emersonii*, shows the carapace free from compression but with an unusually central apex.—Fig. 2. The same, young.—Fig. 3. The same, showing by lateral compression the usual position of the apex and length of the abdominal cleft, as well as the position of the possible dorsal suture.

Lisgocaris, n. g. (λίσγος=a shovel.)

Carapace in one piece, without evidence of dorsal suture. Periphery sub-pentagonal, lateral edges parallel, making sharp angles with the two anterior edges. Anterior edges re-entrantly curved and meeting in the axis of the carapace. As in the genus *Spathiocaris*, there is an abdominal cleft beginning centrally and at the highest point of the carapace, which is considerably elevated, and widening to the posterior margin.

This species, *L. Lutheri*, has the surface of the carapace concentrically marked with fine, crowded, impressed lines. No evidence of abdominal arthromeres. This is a very delicate form measuring three by two millimeters, which has been found near the base of the Hamilton proper in the same horizon as *Estheria pulex*. It belongs to the apus type of the phyllopods with *Peltocaris* Salter, *Discinocaris* Woodward, and *Spathiocaris*.

Northampton, Mass.

Art. LVI.—*An Organ-Pipe Sonometer;* by W. LeConte Stevens.*

While using the sonometer in illustrating the laws of vibration of stretched cords and the nature of musical scales, it has been found desirable to have an instrument on which division-marks were properly arranged to furnish the operator the means of contrasting the scale of equal temperament with the true natural scale, and thus showing to some extent the error of the ordinary keyed instruments. In demonstrating the laws of vibration of columns of air, moreover, it is not always easy to obtain the successive notes of the harmonic series from a single pipe. If the fundamental is pure and strong, either the upper harmonics are unattainable or several are attained at the same moment, and the untrained ear fails to single out the one to which attention is invited. If a single standard of pitch be kept at hand for comparison, the auditor must be practiced in estimating musical intervals; hence a tuning fork alone is not sufficient for most persons. It occurred to me that the union of sonometer with organ pipe in a single instrument would be attended with some advantages, particularly in connection with the exhibition of Bernoulli's laws. The following device has been found very satisfactory.

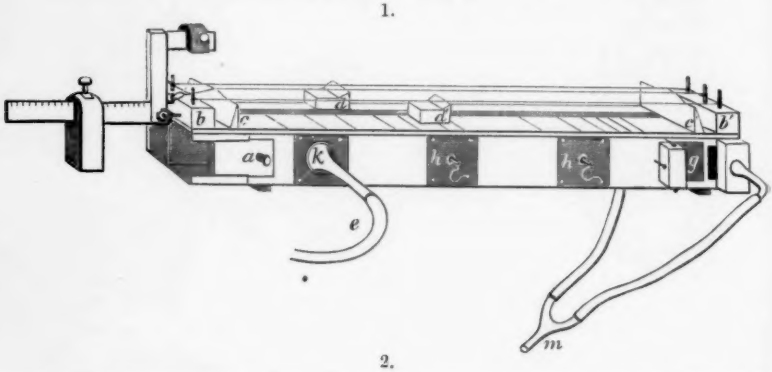
The resonance-box consists of a double organ pipe of spruce, which is rested horizontally on the lecture table. The two embouchures are on opposite sides, one being turned toward the operator, as in fig. 1. The air-blast, from either the lungs or a pair of bellows, may be forced into either one or both pipes at will through gum tubes. The interior dimensions of each pipe are 52^{mm} by 62^{mm}, while the length is adjusted to give as fundamental note, C, 132 vibrations per second. At the open end of one pipe is a sliding plate (a) by which it may be thrown out of unison with the other, thus producing beats. A stop may be thrust, by means of its handle, half way into one of the pair, converting it at will into a stopped pipe whose fundamental is the same as that of the open pipe of double its length.

The upper wall consists of a single plate of spruce, 5^{mm} thick, that forms the sound-board of the sonometer. Firmly fixed at each end is a block of hard wood (b, b') into which piano pins are driven for the attachment of three steel wires which pass over the fixed bridges (c, c'). The latter are exactly 1^{m} apart, in contact with the ends of a strip of wood divided at each edge into millimeters, and occupying the middle of the sound-board. This strip not only serves as a guide for the movable bridges

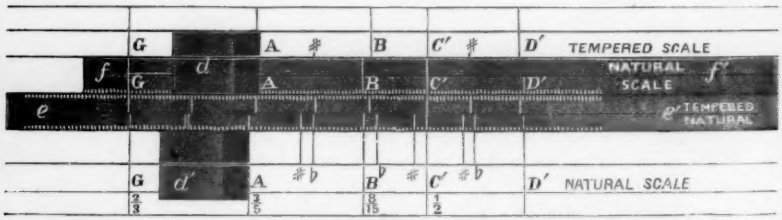
* Presented before the New York Academy of Sciences, May 1st, 1882.

(d, d') but makes it easy to mark off the division lines at their proper places. A longitudinal line divides its surface into two equal parts; that on the side toward the operator is marked off so as to give cord-lengths for the natural scale, by changing the fractions $\frac{8}{9}$, $\frac{4}{5}$, $\frac{3}{4}$, etc., into decimals. The results are indicated, correct to three places, on the millimeter scale. Three octaves in succession are thus marked off; the lines for the first octave

1.



2.



extending across to the edge of the sound-board, as in fig. 2, which represents a part of the instrument viewed from above. By multiplying each of these lengths successively by $\frac{135}{128}$ and $\frac{128}{135}$ we obtain cord-lengths for the chromatic intervals of the natural scale. These are marked off for two octaves, the lines extending half way to the edge. The other side of the central strip (e e', fig. 2) is marked off to give cord-lengths for the successive semitone intervals of the scale of equal temperament. These are easily calculated with the aid of a table of logarithms; for since the twelve intervals of this scale are equal, the successive vibration-numbers form a geometric series, in which the common ratio is

$$r = \sqrt[12]{2} = 1{\cdot}05946.$$

The reciprocal of this is the ratio for cord-lengths, and is

$$r' = {\cdot}94388.$$

Thus, the cord-length for C being one meter, we have for C♯, $l=944^{mm}$; for D, $l=891^{mm}$, etc. Two octaves are thus marked off for the scale of equal temperament, with twelve division-lines to the octave, while on the other side are twenty-one to the octave.

Of the three wires, the two outer ones are kept permanently unisonant, or as nearly so as possible, sounding the same note, C, as the fundamental of the organ-pipes. That in the middle terminates at the left in a stiff ring, so as to be detached in a moment from the hook against which it is ordinarily kept pulled. The ring may then be attached in like manner to a bent lever, such as is employed with the sonometers constructed by Mr. Ritchie, to show the law of variation in tension.

Since the tension of the outer wires is kept constant, each division-mark in both natural and tempered scales, is distinctly labeled, as shown in fig. 2. The movable bridges are black at the front edges so as to contrast strongly with the white spruce on which they slide. Suppose the bridge d' to be adjusted so that its wire sounds E of the natural scale, while its companion, d, is adjusted to give E of the tempered scale. The difference in pitch is nearly a comma, and can be detected by a good ear. But this is additionally made visible by turning the sonometer toward the audience, and resting it on its narrow side; the difference in position of the bridges being easily detectible even when the ear fails to distinguish between the two sounds. Every note in the two scales can thus be compared in a few moments, starting with C as key note.

The necessity for temperament, and yet the unavoidable error of a tempered scale, is still better shown by taking some derived key for purposes of comparison; for example, that of G. For this purpose a separate strip (f, f', fig. 2), properly marked off, is placed at the side of the central strip, the movable bridge being grooved below to slide over it. It is prepared by marking a strip whose length is $\frac{2}{3}$ meter at distances $\frac{8}{9}$, $\frac{4}{5}$, , , , , , , $\frac{1}{2}$, and then discarding the unmarked half. Placing the initial extremity of this scale in contact with the division-mark G of the tempered scale, which in this case sensibly coincides with that of G on the natural scale, it is seen that the second note, if correctly tuned, would be a trifle higher than tempered A, and decidedly higher than natural A; the third would be lower than tempered B, and unisonant with natural B; the fourth unisonant with C, both tempered and natural, etc. Such a strip can be constructed for each one of the derived keys employed in music, though the principle is sufficiently shown with but one.

The division-marks $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, etc., each labeled with the name of the corresponding note, C′, G′, C″, E″, G″, etc., as well

as with the figures composing the fraction, render it easy to obtain with quickness and accuracy the first ten or twelve notes of the harmonic series, besides facilitating experiments on co-vibration.

To obtain the harmonic series from the open organ-pipe, it is found convenient to modify the size of the embouchure and the diameter of the pipe. For this last purpose a piece of wood, 120^{cm} long, 6^{cm} wide, and 2^{cm} thick, is thrust into the pipe next the operator, thus practically diminishing the volume of air set in motion, and increasing the ratio of length to width. A sliding plate (*g*, fig. 1,) of thin sheet-iron serves to narrow the embouchure at will. By varying the force of the blast from the lungs eight or ten successive notes of the harmonic series are thus secured and compared at the same time with those obtained by aid of the wire and labeled scale. The stop can then be thrust into one pipe, and several of the odd series of harmonics elicited.

The wall of the pipe next the operator is perforated with three small holes, at distances $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, from the open end. These are kept stopped with plugs, which may be removed at will. Immediately around them the wall is covered with sheet rubber (*h*, *h*), to secure an air-tight fit for the funnel (*k*) from which a tube (*l*) conveys waves to a manometric capsule. The position of nodal points in the air-column is thus shown. By fitting the tube (*l*) upon a Y-tube like that shown at (*m*), and interposing between this and another Y-tube a pair of rubber tubes, one of which is longer than the other by a half wave-length, the apparatus can be utilized for illustrating interference, with the aid of the manometric flame.

40 West Fortieth street, New York, May 3d, 1882.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *New apparatus for determining Melting Points.*—Cross and Bevan have proposed a simple method for determining melting points which is free from some of the objections of other methods. A small strip of thin sheet iron 9 by 17^{mm} has a hole cut at one end to admit the bulb of the thermometer, which it fits somewhat closely, and contains a small indentation near the other end 1·5^{mm} deep and 2^{mm} in diameter. A glass float is also made, of very fine tube with a small bulb at its lower end, into which is sealed a piece of platinum wire bent at right angles at its extremity. To make an observation, a very small quantity of the substance is melted in the indentation of the plate and while still liquid the platinum wire of the float is placed in it and allowed to become fixed by cooling. The plate is then attached to the thermome-

ter and the whole is immersed in mercury. On heating this slowly, the thermometer being watched, a point is reached at which the substance melts and the float is liberated, rising to the surface of the mercury. Figures are given to show the accuracy of the new method. Purified paraffin fused at 52·5°, resorcinol at 105·0°, mercuric chloride at 258·3° and sodium nitrate at 286·8°—*J. Chem. Soc.*, xli, 111, March, 1882. G. F. B.

2. *On the Critical Temperatures of Organic Liquids.*—Pawlewski has given the results of an extended investigation of the critical temperatures of organic liquids. He finds, (1) that the critical temperatures of homologous compounds differ from their boiling points by a constant quantity; (2) that isomeric ethers have the same or very nearly the same critical temperatures; (3) that bodies in which double union of carbon occurs possess considerably higher critical temperatures than their isomers in which there is no such union, agreeing in this particular with the boiling points; (4) that the critical temperature T_m of a mixture of two liquids can be expressed by the formula $T_m = \frac{nT+(100-n)T'}{100}$, in which n and $100-n$ are the percentages of the constituents and T and T′ their critical temperatures; (5) that the above formula gives the means for determining the critical temperatures of bodies of high boiling points from their more volatile mixtures; since, if the critical temperature and the percentage composition of the mixture are known, as well as the critical temperature of the less volatile constituent, that of the more volatile one is obtained by transposing the formula $T' = \frac{100T_m-nT}{100-n}$; (6) that the percentage composition of mixtures of volatile liquids can be obtained from the same formula, provided the critical temperature of the mixture and of each constituent is known, since $n = 100\left(\frac{T_m-T'}{T-T'}\right)$. Owing to the facility with which water attacks glass at high temperatures, the action beginning even at 240°, the author has not been able to fix very accurately the critical temperature of this liquid.—*Ber. Berl. Chem. Ges.*, xv, 460, March, 1882. G. F. B.

3. *On a new Carbon Sulphobromide.*—Hell and Urech have observed that bromine readily reacts upon carbon disulphide. If a mixture of these substances, best in the proportion of two molecules of the former to one of the latter, be allowed to stand for some days and then the excess of CS_2 be distilled off in a water bath, a brownish-red oily residue is left, which does not solidify if protected from moisture, but which by the action of water becomes a mass of yellow crystals, which can be purified by recrystallization from ether. Alcohol also causes the crystallization. The new body is thus obtained in the form of small, bright prismatic crystals—or if slowly separated, of rhombic plates—having the composition $C_2S_3Br_6$, or carbon trisulpho-hexabromide. It fuses at 125° to a red liquid and solidifies unchanged

on cooling. At higher temperatures it decomposes evolving brown-red condensible fumes and leaving a coal. When pure it has no odor or taste. It is insoluble in water, difficultly soluble in cold ether, alcohol and glacial acetic acid, somewhat more easily in benzene, petroleum naphtha, chloroform, and entirely in CS_2 and bromine. Dilute alkalies in the cold are without action; on heating with NaOH it decomposes as follows:

$$C_9S_8Br_6+(NaOH)_{12}=(Na_2CO_3)_2+Na_2S_3+(NaBr)_6+(H_2O)_6.$$

The author gives four possible constitutional formulas for this body, $S\begin{cases}C\begin{cases}-Br\\=S=Br_2\end{cases}\\C\begin{cases}\equiv S=Br_2\\-Br\end{cases}\end{cases}$ $S\begin{cases}C\begin{cases}=Br_2\\-S-Br\end{cases}\\C\begin{cases}-S-Br\\=Br_2\end{cases}\end{cases}$, $Br_2S\begin{cases}C\begin{cases}=Br_2\\-S\end{cases}\\C\begin{cases}-S\\=Br_2\end{cases}\end{cases}$ and $Br_3{\equiv}C{-}S{-}S{-}S{-}C{\equiv}Br_2$. He gives the preference to the latter on account of the decomposition products. The oily body from which it was produced gave on analysis CS_2Br_4 or $Br_2{=}S{=}C{=}S{=}Br_2$. The crystalline body is therefore a condensation product of the oily one.—*Ber. Berl. Chem. Ges.*, xv, 273, February, 1882. G. F. B.

4. *On a new Sulphur oxychloride.*—Ogier has obtained a new oxychloride of sulphur by heating to 250° in sealed tubes a mixture of equal weights S_2Cl_2 and SO_2Cl_2. The liquid becomes dark red in color and on opening the tubes SO_2 is evolved, and a liquid remains which distills at 60°, and gives on analysis the formula S_2OCl_4. It is formed according to the equation $(S_2Cl_2)_2+(SO_2Cl_2)_2=(S_2OCl_4)_2+SO_2+S$. It is a dark red liquid of density 1·656 at 0°, and of a repulsive odor recalling that of sulphur chloride. Water decomposes it, yielding sulphur, sulphurous and sulphuric acids, hydrogen chloride and some thionic acids. Heat of 100° decomposes it into sulphurous oxide, sulphur chloride and chlorine. Its calculated density is 3·84; observed 3·98, 3·84, 3·75 by Victor Meyer's method; 3·9 by the method of Dumas. The author supposes it to be derived from the hypothetical body S_2O_2 by the substitution of Cl_4 for O_2.—*Bull. Soc. Ch.*, II, xxxvii, 293, April, 1882. G. F. B.

5. *On the Physical Properties of Carbon Oxysulphide.*—The purification of carbon oxysulphide from carbon disulphide is a difficult process. Ilosvay proposes to pass the mixture through a column of freshly calcined charcoal, which totally removes the CS_2. With the gas thus purified the author has determined some of its physical properties. Its expansion-coëfficient was measured by Regnault's method, somewhat modified, both at constant pressure and constant volume. Nine determinations at constant volume, the gas being renewed five times, and the pressure varying from 741·29 to 766·2^{mm}, gave as a mean for $1+100\alpha$, 1·37317. The mean under constant pressure was 1·37908. In the Cailletet apparatus the gas was easily liquefied, the pressure required at various temperatures being as follows: At 0°, liquid at 12·5 at-

mospheres; at 3·8°, 15·0; at 10·7°, 17·5; at 12·0°, 19·6; at 17°, 21·5; at 39·8°, 44; at 41·2°, 45; at 63·0°, 59; at 69°, 65; at 74·6°, 74; at 85°, 80·0. The critical point was found to be at 105°. Liquid COS is colorless, very mobile, highly refractive. Expanded suddenly it freezes in small solid flocks. It dissolves sulphur, and mixes with alcohol and ether, but not with water or glycerin. It is thus intermediate in its properties between CS_2 and CO_2.—*Bull. Soc. Ch.*, II, xxxvii, 294, April, 1882. G. F. B.

6. *On the Formation of Metallic Alloys by Pressure.* — In 1878 and 1880 SPRING showed that many substances when submitted to a pressure of several thousand atmospheres formed masses looking very much as if they had been melted. Moreover he observed that when a less dense allotropic form, as prismatic sulphur, was pressed in this way, it became a more dense variety, octahedral sulphur. Yellow mercuric iodide passes into red solely by pressure. In general a chemical reaction will be effected by pressure provided the resultant is denser than the components, copper and sulphur giving under pressure, cuprous sulphide. Hence the author enunciates the general proposition that matter assumes the state or condition which corresponds to the volume it is forced to occupy. Alloys suggested themselves to him as likely to prove this law, since were the mass after pressure only a mixture, the fusing point would not have changed. A coarse powder of filings of bismuth, cadmium and tin, in the proportions required to form Wood's fusible metal, was prepared and submitted to a pressure of 7500 atmospheres. The block was reduced to filings and again pressed. It now resembled Wood's alloy completely in density, color, hardness, brittleness and fracture. Moreover it melted completely in water at 70°. A similar mixture of lead, bismuth and tin in the proportions of Rose's alloy, after two pressings, melted in boiling water. Zinc and copper filings after five or six pressings gave a mass closely resembling brass. He quotes an experiment by Romna who sought to make a fine platinum wire by drawing it down in a cylinder of silver and then dissolving the silver off with nitric acid, after Wollaston's method. But to his surprise both platinum and silver dissolved in the acid, the pressure in the die-plate having united the two into a definite alloy.—*Ber. Berl. Chem. Ges.*, xv, 595, March, 1882. G. F. B.

7. *On a Symmetrical Chlor-ethyl oxide.*—The isomers aldehyde and ethylene oxide form many analogous compounds, but though the glycol of ethylene has long been known, that of ethylidene has not been prepared. HANRIOT has sought to produce the monochlorhydrin of ethylidene glycol by acting on aldehyde with hydrogen chloride, the latter being passed into the liquid, cooled to 5°, in a slow stream so long as it remains clear. The aldehyde absorbs nearly two-thirds of its weight of HCl. The liquid is then immediately distilled, in vacuo, a vessel of caustic lime being placed between the receiver and the water pump. Under a pressure of 1^{cm} of mercury a liquid passes over boiling

between 25° and 30° having the composition C_2H_5OCl. On standing it becomes turbid and breaks up according to the equation $(C_2H_5OCl)_2=C_4H_9OCl_3+H_2O$, into the ethylidene oxychloride of Lieben and water. The former distills at 52° under a pressure of 4^{cm} of mercury. The first of the above liquids being ethylidene monochlorhydrin $\begin{matrix}CH_3\\|\\CH\\|\\Cl\end{matrix}\left\{\begin{matrix}\\\\OH\end{matrix}\right.$, the author regards the second as symmetrical mono-chlorethyl oxide $\left.\begin{matrix}CH_2CHCl\\CH_2CHCl\end{matrix}\right\}O$, analogous to ethyl oxide $\left.\begin{matrix}CH_2CH_3\\CH_2CH_2\end{matrix}\right\}O$. This constitution he has verified by causing zinc ethyl to react upon the body. The action is prompt and there results $\begin{matrix}CH_3CHCH_2CH_3\\O\\CH_3CHCH_2CH_3\end{matrix}$ secondary butyl oxide. Sodium ethylate gives the same ethylidene oxychloride in which one of the chlorine atoms is replaced by oxethyl.—*Ann. Chim. Phys.*, V, xxv, 219, February, 1882.

G. F. B.

8. *On the Constitution of Quinones.*—Japp and Streatfeild have pointed out the probability that the reaction with aldehyde and ammonia belongs to the class of condensations in the ortho series and that consequently it ought not to be capable of extension to para-quinones. Indeed they suggest this reaction as a proof of the ortho-position in quinones. Having found that both benzoquinone and α-naphthoquinone yield with benzaldehyde and aqueous ammonia, nothing but brown resinous products, they tried chryso-quinone and β-naphthoquinone. Three grams of the former was heated with an excess of benzaldehyde and aqueous ammonia to 100° for one hour in a sealed tube. On solution in hot benzene, silky needles were obtained of the composition $C_{25}H_{15}NO$, being benzenyl-amido-chrysol.—*J. Chem. Soc.*, xli, 157, April, 1882.

G. F. B.

9. *On the Preparation of Alizarin-orange.* — Simon has observed that if dinitro-oxyanthraquinone is suspended in boiling water and very little of a 20 per cent solution of sodium hydrate be added, a deep red solution of the sodium salt is at first formed and then the color passes into purple, and a dark red flocky sodium salt separates. This decomposed with HCl and the yellow residue crystallized from glacial acetic acid, gave beautiful orange-red needles of a mono-nitro-alizarin, β-nitro-alizarin. It fuses at 244°, and dyes orange with alumina. The reaction is: $C_{14}H_5O_2(OH)(NO_2)_2+KOH=KNO_2+C_{14}H_5O_2(NO_2)(OH)_2$. The constitution of the dinitro-body is thus fixed.—*Ber. Berl. Chem. Ges.*, xv, 692, March, 1882.

G. F. B.

10. *Photometric Investigation.*—In this paper E. Keteler and C. Pulfrich continue earlier work (Wied. Ann., xii, p. 481, 1881). The absorption curves of various substances are considered as composed of several curves, and a special study is made of Glan's photometer; the instrument is then applied to a very careful

study of the absorption curves of cyanine, with a view to modifying the theoretical formulas which are used to express the absorption relations. It is found that the extinction coëfficient of a colored solution is closely proportional to the amount of the coloring matter. The study of the composite character of the absorption curves give certain values of the constants used in the theoretical formulas. A portion of the paper is devoted to a reëxamination of the question of the connection between refraction and absorption. —*Ann. der Physik und Chemie*, No. 3, 1882, pp. 337–378. J. T.

11. *Theory of Elliptical double Refraction.*—E. LOMMEL applies the theory of light, previously enunciated by him, to this subject, and finds a satisfactory agreement between theory and experiment. His earlier papers are contained in Wied. Ann., iv, p. 58, 1878; Wied. Ann., xiv, p. 523, 1881.— *Wied. Ann.*, No. 3, pp. 378–391, 1882. J. T.

12. *Difference of Potential between a Metal and Fluids of different Concentration.*—Dr. ERASMUS KITTLER continues his work upon this subject with especial reference to chloride of lithium, chloride of barium, chloride of strontium, chloride of calcium, chloride of magnesium and chloride of manganese.—*Ann. der Physik und Chemie*, No. 3, 1882, pp. 391–412. J. T.

13. *Electrical Resistance of a Vacuum.*—E. EDLUND* reviews the observations of various experimenters upon the electrical resistances of gases in different states of rarefaction, and finds that the phenomena observed can be explained on the hypothesis that a vacuum conducts electricity. With this hypothesis he finds no difficulty in explaining the connection between electrical disturbances on our earth with various phenomena going on on the sun's surface. The celestial bodies are mutually influenced not only by the law of gravitation and light and heat rays, but also by electrical energy. If a vacuum is a good conductor of electricity, an electrical charge upon one planet or star would communicate by induction a corresponding charge in other celestial bodies, and an electrical pulsation would be propagated through space. It appears to the author that the hypothesis that ordinary matter is a necessary medium for electrical action is erroneous; and the terms expressing conductibility therefore lose all physical meaning. Various material bodies oppose a greater or less resistance to the propagation of electrical disturbances. The action of these bodies is passive not active.—*Ann. der Physik und Chemie*, No. 4, 1882, pp. 514–533. J. T.

14. *Solar Heat.*—The engines of Mouchot and others which are designed to utilize solar radiation have suggested the possibility of great economy of fuel. M. A. CROVA gives a short note upon the results obtained by a commission appointed by the minister of public works. The commission carried on the experiments at Montpellier, and used a mirror having a section of $5{\cdot}22^{mq}$ normal to the solar rays, which were concentrated upon a blackened boiler placed at the focus. The boiler was surrounded by a glass

* See also p. 149.

cylinder, and the steam arising from the boiler was condensed in a serpentine condenser. The number of calories utilized by the apparatus was deduced from the weight of water distilled per hour; and hourly actinometric observations gave the number of calories that were received by the apparatus. At the same time the temperature of the air, its hygrometric state, and the heat of the sun were measured; and the thickness of the atmospheric layer traversed was calculated by the formula of Laplace. The number of calories utilized and that received were expressed in great calories (kilogramme-degré) received in one hour upon 1^{mq} of surface normal to the solar rays; their quotient gave the efficiency of the apparatus. The following table expresses the principal results obtained:

Average Value of Results obtained during 1881, *per* 1^{mq} *per* 1^{h}.

	Cal.	Maximum Cal.	
Heat received directly	616·1	945·0	(25 April.)
Heat utilized by apparatus	258·8	547·5	(15 June.)
Mean of efficiency	0·491	0·854	(14 June.)

Under the most favorable circumstances, that is to say, with an incident radiation of 1200 cal. per square meter per hour, which was never obtained, and with an efficiency of eighty per cent, 960 cal. would result. This number represents very nearly the heat produced by 240^{gr} of coal, admitting that nearly one-half of the heat that it produces in burning is utilized in converting water into steam. In reality, in the climate of France not one-half of this efficiency is obtained. The sun does not shine continuously enough to make an apparatus of the description used by the commission practicable. The possibility of its utilization in very hot and dry climates depends upon a number of circumstances, among which enter the difficulty of procuring fuel and the cost of the solar apparatus.—*Comptes Rendus*, No. 14 1882, pp. 943–945.

J. T.

15. *Aconitic Acid, from Sorghum Juices.*—H. B. Parsons (Amer. Chem. Journal, iv, No. 1), has examined the scale which deposits from clear defecated sorghum juice, after treatment with milk of lime to faint alkalinity, on boiling, and which adheres as a light buff-colored deposit tenaciously to the evaporating pans. This scale is easily soluble in hydrochloric acid without effervescence, and the solution reacts abundantly for lime and magnesia but not for glucose or sucrose. Hence it is not a lime sucrate or sulphate, as has been supposed. Analysis showed the presence of lime and magnesium oxides with water and organic matter. Eliminating accidental impurities and considering the scale an impure lime salt, it gave the composition H_2O 7·95, CaO 24·32, organic matter 67·73. The organic acid was separated from the finely pulverized scale by dilute sulphuric acid in quantity short of saturation of the calcium, and salts of zinc, lead, silver, magnesium and calcium respectively found with it and subsequently analyzed with results very closely agreeing to the assumption that the sorghum scale is a two-thirds metallic calcic aconitate

with one molecule of combined water ($CaHC_6H_3O_6 . H_2O$) aconitic acid $=C_6H_6O_6$. Arno Behr has determined the presence of aconitic acid in the melado of the West Indies, (Bericht. der deutsch. Chem. Ges., x, 351). It also exists, as is well known, in the juices of the Monkshood or Wolfsbane (*Aconitum Napellus*), and of Blue rocket (*Delphinium Consolida*) as calcium aconitate, the same salt now found in sugar cane juice and sorghum. B. S.

II. Geology and Natural History.

1. *The Climatic Changes of later Geological Times: a Discussion based on observations made in the Cordilleras of North America;* by J. D. Whitney. Part II. pp. 121 to 264, 4to. Vol. vii, No. 2, Part II, of the Memoirs of the Museum of Comparative Zoology, Cambridge. 1882. Printed for the Museum.—The first part of this memoir, covering 120 pages, was published in 1880, and is noticed in volume xx, of this Journal (p. 460). The view which Professor Whitney partly presented, and supported by facts from North America, in the preceding pages of his memoir—that secular desiccation of the globe, begun as far back as the Cretaceous era, was still in progress—is illustrated further, in this Second part, by facts from "the Asiatic Continent and other portions of the world," after which follows a discussion of the desiccation-question as to its relation to other events, and its possible causes. The facts brought forward are largely those of historical time, which he has gathered with much research from the publications of historians and travelers, but partly from his own personal observations. They are skillfully and fairly presented, making pages of reading full of interest to the general reader not less than to science; for the question whether the world is more or less slowly becoming dried up and unfit for human existence is one of wide concern. The following statements are condensed from its pages.

The region about the Mediterranean in Asia and Africa, and to a less extent in Europe, that is, from the Western Himalayas, westward—the area of the early development of the race, and once inhabited by a dense population that led the world in civilization—has lost its preëminence, and a large part of the people now rank among the semi-barbarous of the world. The causes of this degradation have been many; but one physical cause has made "life no longer possible there except under conditions which are not compatible with density of population or with intellectual vigor"—namely, "the countries in question have become very materially drier than they were during the earlier historic period."

This proposition is sustained by citations from observers as to the diminishing water-surface of the basin of the Aral and Caspian, and the dryness of the table-land of Persia, on account of which the population has diminished and is necessarily diminishing; which dwindling of lakes and drying of streams and land Mr. W. T. Blanford, the geologist of the Anglo-Persian Boundary Com-

mission, has attributed to a gradual reduction in the rainfall of modern times, consequent on a gradual change in the climate of Central Asia. The diminution of lakes and the making the number few where once there were many in Western Thibet and Turkestan is attributed to the same general cause by Von Schlagintweit and the Geological Survey of India—"a progressive decrease of precipitation." The same report is derived from other parts of Asia. Among many other similar accounts it is said of Arabia—"it is a well established fact that the volume of flowing water is constantly diminishing;" of the region bordering on the lower Euphrates and Tigris, once the most fruitful land of antiquity, "the garden of the world," the alluvial bottoms are now covered with plants of a saline soil; of the peninsula of Sinai, "undoubted traces of rich pasturage, of watered ground and of human habitation," were found by Messrs. Tyrwhitt-Drake and Palmer "where all is now a howling waste," and "the area of some 3,000 square miles, formerly so rich in oases, which, thirty-one centuries ago, could send into the field 135,000 swordsmen, is abandoned to a few hundreds of a mongrel Egypto-Bedawi race, half peasants, half nomads;" of Egypt, a similar history; of Greece, as Professor Unger reports—"instead of the former fruitful and well-watered meadows and pastures, now only dry fields and bosky hills devoid of forests are to be found, and, in consequence of this, it is impossible that Greece should ever be again drawn within the circle of Western culture;" and Professor Fraas, recognizing the same cause, says: "On the present soil of the Nile-land never again will any philosophical system be developed, and no power in the world could cause a university to arise there which should have even the most distant resemblance to a European one." The facts of this kind given are very numerous; a few only as examples of them are here cited. The fact of gradual desiccation is further sustained by reports with regard to the Sahara region, Central Africa and South America.

In discussing causes for the desiccation in progress since the Cretaceous era, Professor Whitney sets aside the idea of any effect from the removal of forests, and of any connection with the conditions of the Glacial era, remarking that writers on that era have confined the time of excessive precipitation and its decline to the Glacial and Champlain periods, or the Glacial period only. He then refers to the increase in later geological eras of the extent of land over the globe in proportion to that of water as a real cause but a feeble one, it accounting for only a small part of the diminished precipitation. Finally, he takes up the question —"Has the solar radiation been diminishing in intensity during all or any portion of geological time," and decides that the earth's temperature has been diminishing; and for the reason that the sun has been losing in heating power. In the third part of Professor Whitney's important memoir, yet to be published, he will endeavor to show that "the ideas maintained are not in conflict with the phenomena of the so-called Glacial epoch."

2. *Third Appendix to the Fifth Edition of Dana's System of Mineralogy;* by EDWARD S. DANA. 136 pp. 8vo. New York: (John Wiley & Sons).—This Third Appendix is stated to be designed to make the System complete up to January, 1882. It follows the second Appendix after an interval of seven years, a period which has been remarkable for the unprecedentedly large amount of work done; these facts explain why the present Appendix is double the size of the preceding. It contains full descriptions of all species announced as new, some three hundred in number, and also references to all mineralogical memoirs, published during this period. From the latter many new analyses are quoted and new facts in regard to physical characters and localities. A list of mineralogical works and of mineralogical journals is given in the Introduction.

3. *The Genus Monticulipora.*—Mr. S. A. MILLER has an article in the Journal of the Cincinnati Society of Natural History, for April, 1882 (v, p. 25), showing that the subdivisions of this genus of fossil corals proposed by Professor H. A. Nicholson in his recent memoir, based on specimens from the Cincinnati group, in Ohio, have various objections, and criticising other points in the memoir.

4. *Handbook of Invertebrate Zoology for Laboratories and Seaside Work;* by W. K. BROOKS, Ph.D., Associate in Biology and Director of the Chesapeake Zoological Laboratory of the Johns Hopkins University. 392 pp. 8vo. Boston, 1882. (S. E. Cassino.)—An excellent work, well adapted for its purpose by its judicious arrangement and detailed directions for investigation, and its numerous and admirable illustrations, and attractive also in its letter-press. It is just the hand-book for a sea-side student in zoology, in fact for the learner in zoology anywhere.

5. *The Absorption of Metallic Oxides by Plants,* is the title of an interesting paper read by Professor F. C. PHILLIPS, of Western University, before the Engineers' Society of West Pennsylvania, in February last. It gives the history of what is known and thought about the effect of metallurgical works on adjacent vegetation and the influence of poisonous oxides in the soil; and then adds a summary of experiments made by Professor Phillips upon great numbers of young plants of *Coleus, Achyranthes, Ageratum,* pansies and geraniums, grown in pots for bedding, the object being to determine whether any injurious effects are produced upon plants by being grown in soil impregnated with certain metallic oxides, and whether plants in a perfectly healthy state will absorb such oxides through their roots. The conclusions reached were:

(1.) That healthy plants, grown under favorable conditions, may absorb through their roots small quantities of lead, zinc, copper and arsenic.

(2.) That lead and zinc may enter the tissues in this way without causing any disturbance in the growth, nutrition, and functions of the plant.

(3.) That the compounds of copper and arsenic exert a distinctly poisonous influence, tending, when present in larger quantity, to check the formation of roots, and either killing the plant or so far reducing its vitality as to interfere with nutrition and growth.

In the case of the heavy metals, copper, zinc, arsenic and lead, it seems to be probable that their oxides may under certain circumstances become deposited in the tissues of the plant.

6. *Monographie des Composées*, par H. Baillon. Paris, 1882. pp. 316.— This is the 8th volume of Baillon's *Histoire des Plantes*. One-third of the volume is occupied with an exposition of the floral structure of composite plants, illustrated by the excellent wood cuts which so well set off this and other French botanical books, and with a sketch of the medicinal uses of these plants. In the remaining 212 pages the admitted genera are characterized in large and leaded type, with rather copious bibliographical references in foot-notes. Now Mr. Bentham filled 370 pages of the *Genera Plantarum*—pages of just the same size, but of fine type—with his characters of the tribes and genera of this order. In his laborious and conscientious review of the family, by a botanist who is not at all prone to the multiplication of genera, and who is thought in some cases to have gone too far in reduction, the genera admitted (including a few addenda) amount to 780. The difference in the bulk of the two works is accounted for by the reduction of the genera by Baillon to 403. Baillon's specialty is generic consolidation. Yet withal he keeps up several genera which we find it impossible to maintain; and there are others which should have been suppressed upon his principles, though not upon ours. Deducting these, the genera according to Baillon would be only half as many as those of Bentham and Hooker. Baillon is an able botanist and is entitled to have his own views. If his plan is not adopted, as being neither philosophical nor practically convenient, it is harmless as compared with the opposite extreme; and nothing can be easier than to double up the genera upon a preconceived line after all the materials had been well elaborated beforehand. A. G.

7. *Guide to the Flora of Washington and vicinity*, by Lester F. Ward, A.M. pp. 264, 8vo.—This capital piece of work is published by the U. S. National Museum as Bulletin No. 22, and "one of a series of papers intended to illustrate the collections of natural history and ethnology belonging to the United States and constituting the National Museum, of which the Smithsonian Institution was placed in charge by Act of Congress of August 10, 1846."

The introductory portion treats first of the range of this "flora Columbiana," which is said to be limited on the north by the Great Falls of the Potomac and on the south by the Mt. Vernon estate; it compares the present known flora of the district with that of 1830, as exhibited in the *Floræ Columbianæ Prodromus* by the botanical club of that day; describes in some detail the

localities of special interest to the botanist; indicates the flowering time of the plants and its variation in different years; tabulates and discusses the statistics of the flora; specifies the sixteen largest orders* (these furnishing 55 per cent of the genera and 62 per cent of the species), also the 15 larger genera (beginning with *Carex* and ending with *Asclepias*); mentions the number of introduced species (193, of which 15 are natives of the United States, several indeed hard by), also of the trees (85); institutes comparisons with other floras; and particularly notes the abundant species, those which therefore impress their character upon the landscape. This is a particularly good point; and we are pleased to find that the prominent forest trees, smaller trees, shrubs, woody climbers, and the more conspicuous herbaceous species are in turn enumerated. There are some critical and more explanatory remarks about classification, generally to the purpose. (But it is a mistake to say that, in the New Genera Plantarum, "*Valerianella* has been made coëxtensive with *Fedia*. European authors almost without exception keep up the two genera, it seems to us without good reason. *Steironema* was not adopted in that work only because the authors did not know the peculiar æstivation of the corolla; it was adopted here upon the discovery of this character.) There are good remarks upon common names, i. e. vernacular names; and it is well said "that the best common name for a plant is always its systematic name, and this should be made a substitute for other popular names, whenever and wherever it can be done." We have come to agree with DeCandolle, in urging that while vernacular names are freely to be used in their place, they ought never to be made. A deliberately manufactured vernacular name is a contradiction and a counterfeit. Exceptions there may be, where a generic name may be aptly translated, but these are few.

The flora itself makes up only about 80 pages of this volume. It is a catalogue, without characters or synonymy,—these being at hand in the manuals and proper floras,—but with localities, time of flowering, and many particular observations. It should essentially facilitate and further the study of botany in and around the District of Columbia.

Following the flora is a "check list," with ample room for notes, which occupies 60 pages. The uses of this are explained in the appendix. While the work itself is calculated for the meridian of Washington, the appendix will be very useful wherever there is any botanizing, or any disposition to botanize. It consists of "suggestions to beginners" respecting the identification of plants, collection and preservation of specimens, the making and care of a herbarium, the management of duplicates, of exchanges, and ends with general remarks on herbarium work; all sensible, clear and instructive. Great is the need of such instruction, especially to young United States officers and others who make botanical

* The first eight are *Compositæ*, 53 gen., 149 sp.; *Gramineæ*, 43, 110; *Cyperaceæ*, 10, 108; *Leguminosæ*, 24, 57; *Rosaceæ*, 15, 46; *Labiatæ*, 23, 43.

collections in distant expeditions. The knowledge and the tact they may here acquire should make all the difference between a neat and scientifically valuable collection and one which is repulsive and worthless.

If we rightly understand it, Mr. Ward's plant-poison is not strong enough. We recommend that the alcohol should be saturated with corrosive sublimate, and then, if need be on account of efflorescence, that just a little alcohol be added. And the alcohol should be of the strongest. This appendix covers about the same ground as does Professor Bailey's Botanist's Hand-book, noticed in the preceding number of this Journal. Both are excellent, are needed, and we hope will be widely disseminated. In view of this, it may be expected that the appendix will be separately issued and be distributed by the Smithsonian Institution. A. G.

8. *Les Meilleurs Blés, Description et Culture des principales Variétés des Froments d'Hiver et de Printemps*, par VILMORIN-ANDRIEUX.—This is a handsome imperial 4to, of 175 pages of letter-press and 66 plates, each one admirably representing, in the natural size and colors, two heads of each sort of wheat and some separate grains; not only all the best varieties of *Triticum sativum*, but also of the cultivated spelts and other wheat species, *T. turgidum*, *durum*, *Polonicum*, *Spelta*, *Amyleum*, and *monococcum*. Besides the details for each variety, in a page accompanying the figure, and some general considerations, there is an interesting chapter on the classification of the seven types or species and the many varieties of the common wheat. As this work is a practical one, only the better sorts (according to the title) are here described and illustrated. We may add that this volume comes from the collections and investigations of three generations, son and grandson having continued the collection and the studies commenced by the senior Vilmorin in the year 1820. Louis Vilmorin,—a capital investigator of all such questions,—published his Methodical Catalogue of Wheats in 1850, and died in 1860, since which the work has been prosecuted in the same spirit by the present editor, Henry Vilmorin. A. G.

9. *The Office of Resinous Matters in Plants;* by HUGO DE VRIES. A paper in the *Archives Néerlandaises*, vol. xvii, 1882.—The extract fills 24 pages, 8vo. It has been difficult to make even a plausible conjecture of the uses of the "proper juices" of plants. In their production a large amount of nutritive material is consumed; and for the most part they are stored up irretrievably in the plant, not being reconverted into nutritive material. This gave some color to the old idea that they are excrementitious. But, besides that under normal conditions they are not excreted, why should a pine-tree convert such an amount of its assimilated ternary matters into turpentine, which is merely to be excreted? Or, if it be a bye-product, what useful production or beneficial end attends the production? If excrementitious, the tree should be benefitted by drawing it off. But, as De Vries remarks, and as the owners of the trees very well know, the process is injurious, and if followed up is destructive. It goes almost without saying,

now-a-days, that the turpentine is of real good to the tree, else turpentine-bearing trees would not exist. De Vries has made out a real use, which he thinks is the true function of the resiniferous matters in *Coniferæ*, and in other resin-producing plants. Resinous juice is stored in the tree as *a balm for wounds.* It is stored up under tension, so that it is immediately poured out over an abraded or wounded surface; for these wounds it makes the best of dressing, promptly oxidating as it does into a resinous coating, which excludes the air and wet, and other injurious influences, especially the germs or spores which instigate decay; and so the process of healing, where there is true healing or reparation, or of healthy separation of the dead from the living tissues, is favored in the highest degree. The saturation of the woody layers with resin, in the vicinity of wounds and fractures (as is seen in the light-wood of our hard pines), is referred to as effectively arresting the decay which parasitic fungi set up, this "fat" wood being impervious to mycelium.

Latex or milky juice is a more complex product, of which certain portions have been shown to be nutritive; but as to the caoutchouc and the waxy matters they contain, De Vries insists that they subserve a similar office, are, in fact, a remedy,—a protection against decay, a natural provision for the dressing of wounds, under which healing may most favorably proceed. A. G.

10. *A Popular Californian Flora;* by VOLNEY RATTAN.—This is the "Third Edition, Revised and Enlarged," of a little book which we commended at its first appearance, and which, now that the Botany of California is published, has been somewhat extended. "Introductory Lessons in Structural Botany," with copious woodcuts, are prefixed. So that, all things considered, beginners in botany on the Pacific coast are fairly provided for. A. G.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Astronomical and Meteorological Observations made during the year* 1877 *at the U. S. N. Observatory;* Rear Admiral JOHN RODGERS, Superintendent. Washington, 1881.—This volume contains the results of the regular observations, and also six valuable appendices. (1) Investigation of the Objective and Micrometer of the 26-inch Equatorial, by E. S. Holden; (2) The Multiple Star Σ 748, or the trapezium in Orion, by E. S. Holden; (3) The Solar Parallax from meridian observations of Mars in 1877, by J. R. Eastman; (4) Longitude of the Cincinnati Observatory (viz: $29^m\ 29^s{\cdot}33$ west of Washington), by J. R. Eastman; (5) Longitude of Morrison Observatory, Glasgow, Mo., by J. R. Eastman and H. S. Pritchett (viz: $1^h\ 3^m\ 5^s{\cdot}926$ west of Washington); (6) Observations of Double Stars, by Asaph Hall. Several of these appendices have been noticed in previous numbers of this Journal.

2. *American Association.*—The next meeting—the thirty-first—of the American Association for the Advancement of Science, will be held at Montreal, Canada, commencing August

23. The President of the meeting is Principal J. W. Dawson, of McGill University, Montreal. As now arranged there are nine sections, the Vice-Presidents of which are as follows:—A. Mathematics and Astronomy, WM. HARKNESS; B. Physics, T. C. MENDENHALL; C. Chemistry, H. C. BOLTON; D. Mechanical Science, W. P. TROWBRIDGE; E. Geology and Geography, E. T. COX; F. Biology, W. H. DALL; G. Histology and Microscopy, A. H. TUTTLE; H. Anthropology, DANIEL WILSON; I. Economic Science, and Statistics, E. B. ELLIOTT. A large meeting is expected. The local arrangements are being perfected by a local committee, of which Dr. T. Sterry Hunt is chairman. Of foreign scientists who have been specially invited, the following have signified their intention of being present: SZABÓ of Buda-Pest, RÉNARD of Brussels, STEPHANESCO of Bucharest, W. B. CARPENTER of London, SAMUEL HAUGHTON and FITZGERALD of Dublin, and SOLLAS of Bristol.

3. *Treatise on Astronomy, Spherical and Physical;* by W. A. NORTON. 5th edition revised and enlarged. New York. (Wiley and Sons.)—In this new edition of his well known and much valued work the author has, besides embodying the recent researches and discoveries, added in the appendices new investigations and theoretical discussions in Astronomical Physics.

Abstract of a Report upon the Geology and Mining Industry of Leadville, Colorado, by S. F. EMMONS, Geologist-in-charge Rocky Mountain Division. U. S. Geol. Survey. pp. 203–290, with two colored plates, from the Annual Report of the Director of the U. S. Geol. Survey, transmitted Dec. 1, 1881. Washington, 1882.

Report upon Experiments and Investigations to develop a system of Submarine Mines for defending the Harbors of the United States. Submitted to the Board of Engineers, by Lieut. Col. HENRY L. ABBOT, Corps of Engineers, Brevet Brigadier General, U. S. A., member of the Board. 444 pp. 4to. Washington, 1881. Professional papers of the Corps of Engineers, U. S. A., No. 23.

Insects Injurious to Forest and Shade Trees, by A. S. PACKARD, Jr., M.D. Bulletin No. 7 of the U. S. Entomological Commission, Dept. of the Interior. 276 pp. 8vo. Washington, 1881.

Report on the Meteorology of Tokio for the year 1880, by T. C. MENDENHALL, Ph.D., Professor Experimental Physics in Tokio, Daigaku. Memoirs of the Science Dept. of the Univ. of Tokio. The work contains a thorough discussion of the subject, with numerous tables, charts and diagrams.

Bulletin of the U. S. National Museum, No. 11. Bibliography of the Fishes of the U. S. Pacific coast to the end of 1879, by T. GILL. Washington, 1882.

Results of Meteorological Observations at Providence, R. I., for 45 years, from 1831 to 1876, by ALEXIS CASWELL. Smithsonian Contributions, No. 443. 34 pp. 4to. Washington, D. C., 1882.

Bulletin of the Natural History Society of New Brunswick, St. John, N. B. No. 1. 72 pp. 8vo. 1882.

Descriptions of New Species of Fossils from Ohio, with remarks on the geological formations in which they occur, by R. P. WHITFIELD. March, 1882. Vol. ii. No. 8 of the Annals N. Y. Acad. Sci.

OBITUARY.

SIR CHARLES WYVILLE THOMSON.—Sir Charles Wyville Thomson was born March 5th, 1830, at Bonsyde, Linlithgow, Scotland, where he died March 15, 1882. His father, a surgeon in the service of the East India Company, intended to make him a physician. But the medical profession had no charm for him except

as it concerned the study of nature, and he gave up the thought of practice and accepted a lectureship on botany, first at King's College, Aberdeen, and afterward at Marischal College. During the two years of his lectureship he devoted much of his time to the study of the Invertebrates, and at the age of twenty-three he was appointed Professor of Natural History at Queen's College, Cork. Here he remained but a year, being called in 1854 to the Professorship of Mineralogy and Geology at Queen's College, Belfast, a position which he occupied till he became the successor of Professor Allman as Regius Professor of Natural History in the University of Edinburgh. This post he resigned a few months before his death on account of ill-health.

The versatility of Professor Thomson's attainments is shown by the variety of learned posts, all of which he filled with marked distinction. While at Belfast he wrote the greater number of those zoological memoirs which gave him an eminent rank among naturalists. Thoroughly devoted to the interests of Belfast, he brought together collections for the college there which were of value and importance. In the meantime Thomson's interest was early and strongly aroused by the investigations of the Norwegian and American naturalists into the fauna inhabiting the depths of the sea. Satisfied that so ardent and able a naturalist could not fail to turn any means placed in his hands to the best account, the government was induced to give him in successive years the "Lightning" and the "Porcupine," and finally the "Challenger" and the "Knight Errant." The first and second of these explorations into that submarine world which is the land of promise of the naturalist, were directed by Dr. Wm. B. Carpenter, Mr. Jeffreys and Professor Thomson, and the results were embodied by Thomson in a most attractive volume entitled, "The Depths of the Sea," by far the most interesting account which has ever appeared of the new zoological problems suggested by similar expeditions. Stimulated by the success of these two voyages of discovery, the Royal Society of London appointed a committee to urge upon the government the wisdom of fitting out another expedition on a gigantic scale. To this the government acceded, and in 1873 the "Challenger" started on her memorable voyage. The scientific direction of the expedition was intrusted to Thomson and for three years he and his colleagues were at work searching the depths of the Atlantic, the Pacific, and extending their researches far toward the Antarctic pole. The cruise was in every way successful, broken by only one untoward event, the death of Dr. R. von Willemoes Suhm. During this famous expedition the "Challenger" sailed over 70,000 miles of sea, pausing at 362 stations, at each of which, systematic observations were made by the naval officers and the civilian staff of investigators. Not only were most extensive collections made illustrating the novel and wonderful fauna characteristic of the ocean depths, but physical observations of the greatest importance were taken, and materials were brought

together for the first hydrographic sketch map of the great oceanic basins, as well as records and measurements of temperatures and currents wherever such were feasible.

These immense collections were sent home from time to time, and on the return of the expedition the working up of the whole mass of material was entrusted to its scientific director, Sir Charles Wyville Thomson. He selected as his assistants the best known specialists in the different branches of the animal kingdom, and three volumes of the zoology have already appeared. Unhappily Thomson's own part of the work is but little advanced, and the crinoids and sponges which he hoped to work up himself must now pass into other hands. Only two volumes of his general account of the voyage of the "Challenger" have been published, under the title of "The Atlantic." The material for the other intended volumes on the Pacific and the Southern ocean exist only in the form of notes, and in the sketch of the results of the "Challenger" expedition, given by Thomson in his address as President of the Geographical Section of the British Association at its Dublin meeting.

The death of such a man at a comparatively early age, before he had time to gather in the harvest he had so fairly won, is not only sad in itself but is a great loss to science. His extensive and exact knowledge in many fields of biology would have enabled him to put together the varied results of his expedition as no one else can do it.

Thomson was a general favorite, beloved not only by his colleagues but by his students who flocked to his lectures. Natural History was perhaps never more popular than while he held the professorship, which had been occupied in turn by Jamieson, Forbes and Allman. His relations with his foreign associates were always frank and kindly, and those who had the good fortune to meet him parted from him not only as from an old friend, but also with sincere admiration for his character and extensive attainments. A. AG.

General JOHN G. BARNARD, of the Department of Engineers, U. S. Army, died on the 14th of May. General Barnard was an able mathematician, and the author of papers on Projectiles, the Gyroscope, the Tides and other subjects.

CHARLES M. WHEATLEY, of Phenixville, Pennsylvania, Mining Engineer, died on the 6th of May. Mr. Wheatley's discoveries of a Saurian bone-bed near Phenixville in the Mesozoic shales of the region, and of a Quaternary Cave in Eastern Pennsylvania containing bones of the Mastodon, Megalonyx and other extinct species, were of the highest interest to American geology.

WILLIAM S. VAUX died at Philadelphia on the 5th of May. Mr. Vaux used his wealth largely for scientific purposes. He gathered one of the finest collections of minerals in the country, and besides a large archæological collection. Among his bequests, there is one of $10,000 to the Academy of Natural Sciences, and another, of his Collection of Etruscan Pottery, to the Pennsylvania Museum and School of Industrial Art.

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